PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From February 25, 1897, *to June* 17, 1897.

VOL. LXI.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

February 25, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Note on the Dielectric Constant of Ice and Alcohol at very Low Temperatures." By JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, London, and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London.

II. "On the Relation between Magnetic Stress and Magnetic Deformation in Nickel." By E. TAYLOR JONES, D.Sc. Communicated by Professor ANDREW GRAY, F.R.S.

III. "On the Relations between the Cerebellar and other Centres (namely Cerebral and Spinal) with especial reference to the Action of Antagonistic Muscles. (Preliminary Account.)" By MAX LÖWENTHAL, M.D. (Würz.), M.R.C.P., and VICTOR HORSLEY, F.R.S., F.R.C.S.

IV. "On the Action of Light on Diastase and its Biological Significance." By J. REYNOLDS GREEN, Sc.D., F.R.S., Professor of Botany to the Pharmaceutical Society of Great Britain.

V. "Fragmentation in *Lineus gesserensis*." By ALEX. BROWN, M.B., B.Sc., M.A., Lecturer in Zoology, and Senior Assistant in the Natural History Department, University of Aberdeen. Communicated by Professor MCINTOSH, F.R.S.

"Note on the Dielectric Constant of Ice and Alcohol at very Low Temperatures." By JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, London, and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London. Received January 27,—Read February 25, 1897.

Of late years many careful determinations have been made of the dielectric constants of water and ice by different observers. These evaluations may be divided into two classes. Firstly, those which are, strictly speaking, determinations of the specific inductive capacity of the material, and have been made by measuring the change in the capacity of a condenser when water or ice is substituted for air as the dielectric. Secondly, those which are really measurements of the electrical refractive index of water or ice for electric waves of various lengths, and which have been generally made by obtaining the reduction in wave-length experienced by an electric wave on passing from air into water or ice. The square of this refractive index or the ratio of wave-length reduction is then taken as the dielectric constant.

In order that results thus recently obtained may be compared, we have collected into two tables (I and II) some of the values for the specific inductive capacity, and the electric refractive index of water for waves of various lengths.

The determinations of the specific inductive capacity quoted all appear to have been made by methods which, whilst excluding, or believed to exclude, error arising from the conductivity of the water, may yet be regarded as giving the value corresponding to comparatively slow reversals of electromotive force or to waves of infinite wave-length. The electric refractive index observations have been made by using electric waves of lengths in air varying from 8 mm. to 600 cm.

Two very careful determinations of the specific inductive capacity of water seem to be those of W. Nernst (80·0 at 17° C.) and F. Heerwagen (= 80·88 at 17° C.), and that of J. F. Smale is in close agreement with that of Nernst.

As regards the electrical refractive index, it will be seen that the determinations of P. Drude for waves of 70 cm., which give $\mu = 8{\cdot}95$, and hence $\mu^2 = 80{\cdot}2$ at 17° C., and those of Cohn and Zeeman, which give $\mu = 8{\cdot}91$ and $\mu^2 = 79{\cdot}39$, as a mean value for waves from 155 to 560 cm. in length, are in fairly close agreement with one another, and with the best determinations of specific inductive capacity.

Table I.—Determinations of the Dielectric Constant (Specific Inductive Capacity) (K) of Water by Various Methods.

Observer.	Reference.	Value found.	Method.
W. Nernst ...	'Zeits. phys. Chem.,' 1894, vol. 14, pp. 622—633	80·00 at 17° C.	By condensers balanced on a Wheatstone's bridge arrangement using a telephone as detector.
C. B. Thwing	'Zeits. phys. Chem.,' 1894, vol. 14, pp. 286—300	75·5	By resonance of two electrical circuits. Capacity in each adjusted to identity.
L. Graetz and L. Fomm	'Wied. Ann.,' 1895, vol. 54, pp. 626—640	73·54	By deflection of a dielectric ellipsoid suspended in water in an electric field.
T. F. Smale ..	'Wied. Ann.,' 1896, vol. 57, pp. 215—222	80·05	By using an electrometer filled with water.
F. Heerwagen	'Wied. Ann.,' 1893, vol. 49, p. 279	80·88 at 17° C.	By using a double electrometer, one filled with water. Reversals of polarity 42—85 per second.
A. Franke....	'Wied. Ann.,' 1893, vol 50, p. 163	81·65 at 17° C.	By using a double electrometer and reversals of polarity made by an induction coil.
W. K. Röntgen	'Wied. Ann.,' 1894, vol. 52, pp. 593—606	86·0	By alternate currents and condenser.
E. B. Rosa ...	'Phil. Mag.,' 5th ser., vol. 31, 1891, p. 188	75·7 at 25° C.	By attraction of plates of a condenser, alternating potentials used. 2000 to 4000 reversals per minute.

The general results show that the square root of the specific inductive capacity of water as determined by relatively slow-speed electrostatic methods is expressed by a number which is not very different from that which denotes the refractive index of water for electrical waves varying in length from 8 mm. to 600 cm. Maxwell's law is, therefore, fulfilled in the case of water under these conditions.

The general evidence at disposal does not indicate any very marked dispersive power on the part of water for electric waves varying in length between the above-named limits; though the careful results of P. Drude in 1896, for waves from 40 to 200 cm. in length, taken by themselves, indicate a slight normal dispersion, the refractive index increasing with decreasing wave-length. Change of temperature has a marked effect upon the electrical refractive index, and conclusions cannot be drawn, therefore, from the comparison of observations not made at the same temperature.

There is, however, no such good agreement between the results of these two classes of physical measurement in the case of ice.

Table II.—Determinations of the Electrical Refractive Index (μ) of Water for Electric Waves of Various Wave-lengths.

Observer.	Reference.	Value found.			Wave-length of waves used in air.	Method.
		μ.		μ^2.		
P. Drude.....	'Wied. Ann.,' 1895, vol. 54, pp. 352—370	8·9		79·21	300—600 cm.	Measured reduction of wave-length on passing from air to water.
P. Drude.....	'Wied. Ann.,' 1895, vol. 55, pp. 633—655	8·7		75·69	72 cm.	Measured reduction of wave-length on passing from air to water.
P. Drude.....	'Wied. Ann.,' 1896, vol. 58, pp. 1—20	9·07	(11·7° C.)	82·21	70 ,,	Measured reduction of wave-length on passing from air to water. Last result best.
		8·95	(17° C.)	80·2		
H. Rubens and A. D. Cole	'Ver. phys. Ges., Berlin,' 1895, vol. 14, pp. 76—78	8·8		77·04	5 ,,	Measured intensity of rays reflected from surface of water at incidence of 45° and calculated μ by applying Fresnel's formulæ.
		8·9		79·21		
A. D. Cole	'Wied. Ann.,' 1896, vol. 57, pp. 290—310	8·85		78·32	5 ,,	Same as above.
V. von Lang.....	'Wien. Sitzungsber.,' vol. 105, Part IIa, p. 253	9·4		88·36	8·5 ,,	By interference of electric waves.
P. Drude.....	'Wied. Ann.,' 1896, vol. 59, pp. 17—62	9·14		83·6	40 ,,	Measured reduction of wave-length in passing from air to water. Found μ varied with temperature. $\mu^2 = 88{\cdot}23 - 0{\cdot}4044t + 0{\cdot}001035t^2$.
		9·03		81·67	75 ,,	
		8·98		80·60	200 ,,	
E. Cohn and P. Zeeman	'Akad. d. Wissensch. zu Amsterdam,' Sept., 1895	8·91		79·39	155 ,,	Measured reduction in wave-length in passing from air to water.
					350 ,,	
					560 ,,	
A. Lampa.....	'Wien. Sitzungsber.,' 1896	8·972		80·49	8 mm.	
D. Mazzotto	'Rend. Acc. Linc.,' 5, 2, p. 301, 1896.	9·0	(19° C.)	81·0	234—1836 cm.	By use of Lecher apparatus. Measured reduction of wave-length in passing from air to water.

M. E. Bouty ('Journal de Physique,' 3 S., vol. 1, 1892, p. 454) found by a slow-speed method a value of 78·8 for the specific inductive capacity of ice at −23° C. and upwards, whilst R. Blondlot ('Compt. Rend.,' vol. 119, 1894, pp. 595—597), using electric waves and measuring the wave-lengths in air and in ice, found 1·41 for the electric refractive index of ice, and hence the number 2·0 as the value of the dielectric constant of ice at a temperature not stated, but presumably not far below 0° C.

Also A. Perrott ('Compt. Rend.,' vol. 119, 1894, p. 601, also 'Compt. Rend.,' vol. 114, June, 1892, p. 1528) found the value 1·43 for the refractive index, and hence 2·04 for the square of the electric and refractive index of ice. C. B. Thwing, working with electric resonance, and therefore very rapid reversals, gives ('Zeits. Phys. Chem.,' vol. 14, 1894, pp. 286—300) 3·36 at −2° and 2·85 at −5° C. as values for the dielectric constant of ice.

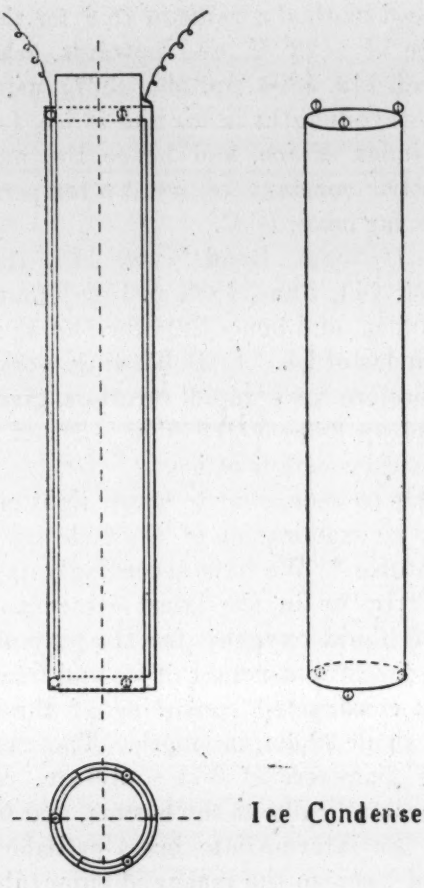
It seemed desirable to endeavour to throw light on the reasons for these differences by an examination of the dielectric constant of ice at very low temperatures.* We have accordingly applied the method and apparatus used by us in the latest determinations of the dielectric constant of liquid oxygen† for the purpose of making an examination of the dielectric constant of ice and frozen ethyl alcohol.

A condenser was constructed consisting of three very thin concentric brass tubes about 20 cm. in length. The extreme outer and inner tubes, having diameters of 3·74 and 3 cm. respectively, were connected together metallically at the bottom, and formed one plate of the condenser. An intermediate brass cylinder 3·37 cm. in diameter was suspended between the inner and outer tubes, thus forming the other plate of the condenser. This last plate was insulated and suspended from the others by providing it with small wire attachments, which were passed through the holes in six glass beads wedged in between the inner and outer connected cylinders at the top and bottom as shown in fig. 1. This condenser was contained in another brass cylinder closed at the bottom, and then had its dielectric formed of ice by filling the annular space between the cylinders with distilled water and freezing it. The terminal wires from this condenser were connected to a rapid contact maker driven by an

* *Note added February* 15, 1897. Since this Paper was presented a communication has been made to the Royal Society by Dr. J. Hopkinson and Mr. E. Wilson, in which they describe experiments on the dielectric constant of ice. These authors find that the specific inductive capacity of ice, when measured with electromotive force reversals having a period of from 1/100 to 1/10 of a second, is a number of the order of 80, but if measured with periods such as $1/10^6$ of a second it is a number less than 3. The difference they ascribe to residual charge. Hence it is clear that these observations are in accord with those of Bouty and Blondlot.

† Fleming and Dewar, "On the Dielectric Constant of Liquid Oxygen," 'Roy. Soc. Proc.,' 1896, vol. 60, p. 358. See note added December 18, 1896, p. 368.

FIG. 1.



Ice Condenser.

electrically controlled tuning fork. The tuning fork used made 124 complete vibrations a second, and was made, by means of a mercury cap and steel stylus dipping into it, to close an electrical circuit 124 times every second, and thus drive synchronously an electro-magnetic contact maker, which placed one terminal of the condenser alternately in connection with a battery of fifty lithanode secondary cells, and with a sensitive galvanometer. The other terminals of the battery, galvanometer and condenser, were connected together. In this way the galvanometer was traversed by a rapid series of electric charges, which have all the effect of a continuous current. The galvanometer deflection remains perfectly steady as long as the battery voltage is unaltered. Other things remaining the same, the galvanometer deflection measures the capacity of the condenser. In employing this method, the galvanometer may be arranged so as to be affected by the series of *discharges* of the condenser, or it may be placed so as to be traversed by the series of *charges* of the condenser. If the condenser has any sensible leakage or dielectric conductivity

this will show itself by making the galvanometer readings in the two cases unequal. Mr. Petavel, who assisted us in these observations, and to whom our thanks are due, arranged a convenient switching device which enabled the galvanometer to have its position in the circuits instantly changed to take either the charge currents or the discharge currents of the condenser, and the equality of these readings is taken as an indication that no sensible leakage takes place across the dielectric during the passage of the contact maker from one stop to the other. This method of exchanging the position of the galvanometer also eliminates errors due to the setting of the scale, as the deflections are on opposite sides of the zero. The above-described arrangements having been made, the ice condenser was cooled down to the temperature of liquid air by immersing it in the liquefied gas contained in a large vacuum vessel.

In order to take the temperature of the condenser a platinum wire resistance thermometer was placed in the inside of the inner cylinder and in close contact with it. The ice having been reduced in temperature to −185° C., or −198° platinum temperature, the capacity of the condenser was measured. The condenser was then raised out of the liquid air and allowed to warm up very slowly, and its capacity taken at various stages as the temperature rose. Before and after the experiment with the ice the capacity of the condenser was taken when the metal cylinders were at the temperature of −185°, but the dielectric was gaseous air at that temperature instead of ice, and the results so obtained enabled the dielectric constant of the ice to be calculated.

These experiments being to a considerable extent preliminary experiments, and intended merely to explore the ground, we do not make any particular claim for the accuracy of the numbers as determinations of a physical constant. We are arranging improved methods for repeating the whole of these measurements. All we desired in the first instance to do was to examine the mode in which the dielectric constant varied with temperature and its approximate magnitude at each temperature. The following Table III shows the observed value of the dielectric constant of ice between −185° C. (−198° pt.) and about −120° (−130° pt.). The voltage used on the condenser was 24·1 volts, and kept perfectly constant during the whole time. In order to ascertain if the galvanometer deflection was really due to the sequence of capacity charges or discharges only, and not to any measurable admixture of conduction current through the ice, a resistance of 1,000 to 90,000 ohms was occasionally inserted in the galvanometer circuit, and the absence of any observed change in the galvanometer deflection was taken as a proof that sensible conduction did not interfere with the true capacity effect. The galvanometer used had a resistance of 500 ohms.

In the following table the temperatures are given in terms of platinum temperature (pt.) as defined by our standard platinum thermometer P_1.*

Table III.—Dielectric Constant of Ice at Various Temperatures. Frequency of Fork, 124.

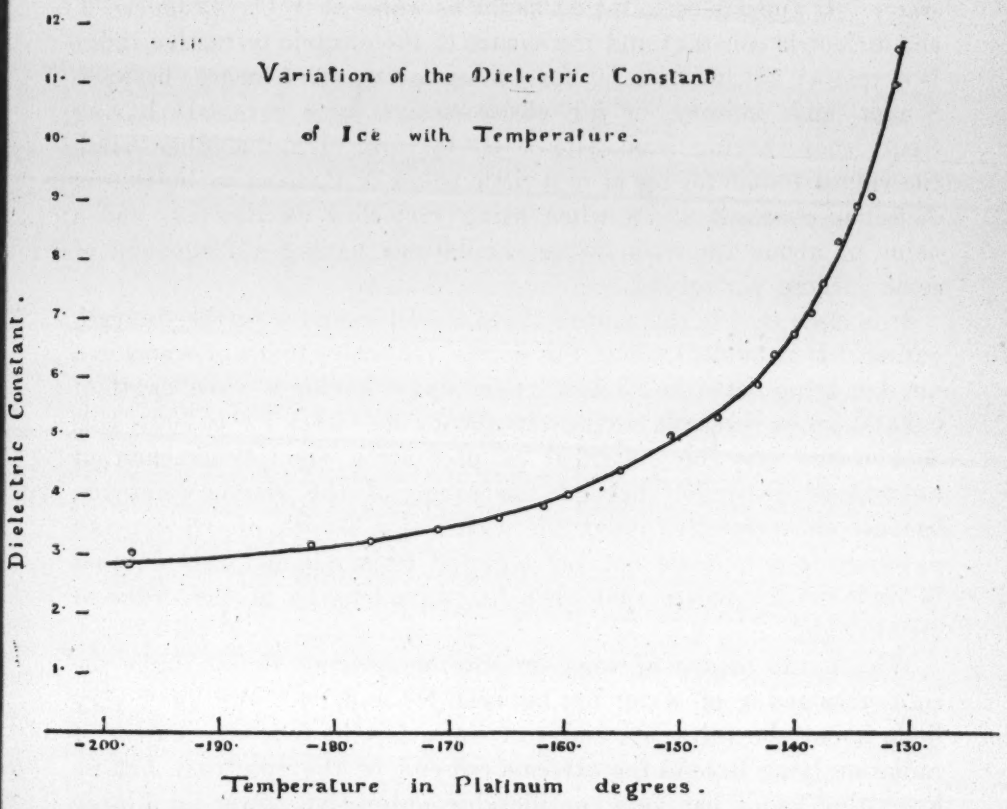
Temperature in platinum degrees by standard thermometer P_1.	Scale reading or galvanometer deflection in centimetres.	Dielectric constant of ice.
pt.		
−198·0	5·6	2·83
−197·8	5·6	2·83
−197·6	6·0	3·03
−182·0	6·25	3·16
−176·8	6·4	3·23
−171·0	6·8	3·44
−165·7	7·2	3·64
−161·8	7·6	3·84
−159·7	8·0	4·04
−155·2	8·8	4·45
−150·8	10·0	5·05
−146·7	10·6	5·36
−143·2	11·7	5·92
−141·7	12·7	6·42
−140·0	13·4	6·77
−138·5	14·1	7·13
−137·5	15·1	7·63
−136·2	16·5	8·34
−134·5	17·7	8·94
−133·2	18·0	9·1
−131·0	21·8	11·0
−130·7	23·0	11·6

With the above frequency (124) the conductivity of the ice began to make itself felt at about −130° pt.

The above numerical results are plotted out in a curve in fig. 2. They show clearly that the dielectric constant of the ice increases progressively from a value of about 2·8 to 11·6 between the limits of −198° and −131° pt. Moreover, the trend of the curve shows that at the absolute zero the dielectric constant of the ice would probably not be far from 2·0. With the above arrangements we were not able, partly for want of time and partly because of the very moderate frequency of the tuning-fork, to take the dielectric constant at higher temperature, but this we hope to do before long, and also to examine the effect of varying the frequency of the contact maker.

* See Dewar and Fleming, "On the Thermo-electric Power of Metals and Alloys," 'Phil. Mag.,' July, 1895, p. 100.

FIG. 2.



The broad general result which emerges from these experiments is that at a temperature of −185° C. we find a value for the dielectric constant for ice, when using relatively very slow reversals of electromotive force, which is not very different from that found by observers using reversals of many millions per second by the use of electrical oscillations or waves, when working at temperatures of 0° C. or a little below.

C. B. Thwing ('Zeits. Phys. Chem.,' vol. 14, 1894), using an electrical resonance method, has examined the variation of the dielectric constant of water with temperatures from 0° C. to 88° C., and found a maximum value of 85·2 at or near the temperature of maximum density of water.

It seems therefore to be a matter of some importance to measure the dielectric constant of ice at all temperatures from the lowest which can be reached up to 0° C., using various frequencies of alternating electromotive force, and to explore the mode of variation of the dielectric constant with temperature and with frequency throughout this range. The point which especially needs to be cleared up is whether the dielectric constant of ice is, or is not, more changed by

change of frequency of the electromotive force than is the case with water. It appears certain that as far as water at 0° C. is concerned, the dielectric constant and the square of the electric refractive index is a number not far from 80, for waves having wave-lengths between 8 mm. and infinity, or for electromotive force reversals having frequencies varying from $37{\cdot}5 \times 10^9$ to zero. On the other hand, the values found for ice at or a little below 0° C. seem to indicate a dielectric constant of 78, when using very slow oscillations; and a value of about 2·0 when using oscillations having a frequency of some millions per second.

It is clear that in this matter there is still room for further investigation. It is evident, since the optical refractive index of water is a number lying between 1·3 and 1·4 for waves having a wave-length of 0·00005 cm. or reversals having a frequency of -400×10^{12} to 700×10^{12}, that water may be regarded as presenting the phenomenon of anomalous dispersion beyond the range of the visible spectrum, because the refractive index for waves of a length of 0·8 cm. and upwards is a number not far removed from 8·9, and this number is very much greater than that for wave-lengths of the order of visible light.

Within the octave of wave-lengths comprising visible light the refractive index of water lies between 1·3 and 1·4. We know very little about the refractive index of water for the fourteen octaves of radiation lying beyond the extreme red end of the spectrum, but we know that water has very considerable absorptive power for a large range of this radiation. The next ten octaves beyond the last, include the range of the Hertz radiation or of wave-lengths from $\frac{1}{2}$ to 500 cm. in length, and for all this the refractive index of water is approximately 8·9. It remains to be seen how the high value is connected with the low one, and whether this variation may be properly regarded as a case of anomalous dispersion analogous to that found in the case of an alcoholic solution of fuchsine within the range of the visible spectrum. It is evident that since the dielectric constant of any one substance, such as ice-water, is a function both of temperature and time, it can best be represented geometrically by a *surface*, which may be called the dielectric surface, and which is defined by the co-ordinates representing dielectric constant, temperature, and frequency of electromotive force reversals.

The details of two determinations of the dielectric constant of ice at −185° C. are given in Table IV.

The same readings were obtained both with 90,000 ohms and 1,000 ohms in the galvanometer circuit.

The above figures of observation require two corrections to be applied. In the first place, the pins which support the inner condenser plate, and which pass through glass beads, have a total area

Table IV.—Brass Condenser filled with Distilled Water and frozen into Ice. Ice reduced to −185° C. by use of Liquid Air.

Deflections of galvanometer when in charge circuit.	Deflections of galvanometer when in discharge circuit.	Volts on terminals of condenser.	Mean galvanometer deflection.
22·4 cm.	23·45 cm.	101·1 volts	22·93 cm.
22·3 „	23·50 „	101·1 „	22·90 „
22·35 „	23·50 „	101·1 „	22·92 „

Mean galvanometer deflection = 22·917. Frequency of contact maker 124.
Mean voltage on condenser = 101·1.

of 0·4 sq. cm., or 0·17 per cent. of the area of the condenser plate, and taking the specific inductive capacity of the glass as equal to 5·0 when cooled to −185° C., it can be seen that the glass supports form 0·85 per cent. of the effective capacity of the condenser when the dielectric is gaseous air at −185° C.

In order to determine this last capacity the brass condenser had its capacity taken at 20° C., air being the dielectric, and then it was cooled in liquid air and then lifted out into the cold gaseous air at a temperature of −185° C., lying above the liquid air, and in this condition the same readings taken. The following were the observed readings of the galvanometer:—

Table V.—Brass Condenser at 20° C., filled with Gaseous Air at that temperature. Frequency of Contact Maker, 124.

Deflection of galvanometer when in charge circuit.	Deflection of galvanometer when in discharge circuit.	Volts on terminals of condenser.	Mean galvanometer deflection.
8·32 cm.	8·31 cm.	101·0 volts.	8·315 cm.
8·35 „	8·30 „	101·1 „	8·325 „

Table VI.—Brass Condenser at −185° C., filled with Gaseous Air at that temperature. Frequency of Contact Maker, 124.

Deflection of galvanometer when in charge circuit.	Deflection of galvanometer when in discharge circuit.	Volts on terminals of condenser.	Mean galvanometer deflection.
8·30 cm.	8·48 cm.	101·1 volts	8·39 cm.

The difference between the mean galvanometer readings, viz., 8·31 and 8·39, in the two last cases, is due to the fact that the condenser capacity is changed by the cooling, owing to the fact that the plates contract. The actual difference due to the temperature of the air changing from 20° C. to −185° C. is very much smaller, and does not sensibly affect the capacity as far as the accuracy of the present measurements is concerned.

There is, however, another correction which needs to be applied, and that is for the capacity of the leads and of the contact maker. Experiment showed that this was equivalent to 4·15 mm. deflection of the galvanometer, and hence the amount has to be deducted from all mean galvanometer readings.

The final result then is as follows :—

The specific inductive capacity, K, of the ice at −185° C. is very nearly equal to the ratio of (22·917−0·07−0·415) to (8·39−0·07−0·415), or K = 2·83.

The correction 0·07 is the correction for the capacity of the glass separators, which is equal to $5 \times 0{\cdot}17 = 0{\cdot}85$ per cent. of the total capacity, and hence the remanent capacity of the condenser is $8{\cdot}39 - 8{\cdot}39 \times 0{\cdot}0085 = 8{\cdot}39 - 0{\cdot}07$. The correction 0·415 is the correction for the capacity of the contact maker and leads.

A second determination was made at a lower voltage, the details of which are as follows in Table VII.

Table VII.—Brass Condenser filled with Distilled Water and frozen into Ice. Ice reduced to −185° C. by use of Liquid Air.

Deflection of galvanometer when in charge circuit.	Deflection of galvanometer when in discharge circuit.	Volts on terminals of condenser.	Mean galvanometer deflection.
8·35 cm.	8·60 cm.	36·1 volts	8·48 cm.
8·35 „	8·60 „	36·1 „	8·48 „

Mean galvanometer deflection = 8·48 cm.
Mean voltage = 36·1 volts.
Mean deflection for voltage of 101·1 = 23·75 cm.
Corrected value of the deflection = 23·75−0·07 = 23·68.

Specific inductive capacity of ice at −185° C. $= \dfrac{23{\cdot}68 - 0{\cdot}415}{8{\cdot}32 - 0{\cdot}415} = 2{\cdot}94.$

Hence the mean value of the dielectric constant of ice at −185° C. for slow reversals of electromotive force is a number not far from 2·9.

We have in the same manner examined ethylic alcohol frozen and reduced to −185° C.

The observations with same condenser having frozen alcohol as the dielectric are as follows :—

Table VIII.—Brass Condenser filled with Absolute Ethylic Alcohol frozen and reduced to −185° C.

Deflection of galvanometer when in charge circuit.	Deflection of galvanometer when in discharge circuit.	Voltage on terminals of condenser.	Mean galvanometer deflection.
12·20 cm.	12·80 cm.	50·5 volts	12·5 cm.
12·10 „	12·50 „	„ „	12·3 „
12·10 „	12·50 „	„ „	12·3 „

Mean galvanometer deflection = 12·60.
Mean voltage = 50·5.

Galvanometer deflection corrected for capacity of glass separators and also of contact breaker, and reduced to correspond to a voltage of 101·1 = 24·665.

Since the corrected capacity of the condenser when at −185° C. and filled with gaseous air at −185° C. is represented by the number 7·905 = 8·39−0·07−0·415, we have for the dielectric constant of solid alcohol at −185° C. the value 24·665/7·905 = 3·12.

In addition to measuring the dielectric constants, the same experimental arrangements enabled us to measure approximately the dielectric resistance of the ice and frozen alcohol at and from the temperature of −185° C., and the following tables give the approximate total resistance of the condenser when the dielectric consisted of these substances and was slowly allowed to heat up from −185° C.

Table IX.—Variation in the Resistance of an Ice Condenser heated up from −185° C. = −200° pt. to about 70° C. Temperatures given in platinum degrees by standard platinum thermometer P_1.

Temperature of the ice in platinum degrees.	Resistance of the ice condenser in megohms.
−200·0°	26200·0
−172·2	5670·0
−135·0	1570·0
−126·0	1130·0
−108·4	706·0
−98·8	470·0
−95·2	353·0
−93·2	282·0

Temperature of the ice in platinum degrees.	Resistance of the ice condenser in megohms.
−91·8	209·0
−88·8	118·0
−88·2	91·4
−86·3	66·5
−84·4	53·4
−82·3	46·3
−75·0	42·8
−70·7	43·4

In the same way the resistance of the frozen ethylic alcohol condenser was taken at temperatures lying between −185° C., and about −160 C., as given in the table below.

Table X.—Variation in Resistance of the Frozen Alcohol Condenser Temperatures given in Platinum Degrees.

Temperature of the alcohol in platinum degrees.	Resistance of condenser in megohms.
−200·0°	14500·0
−190·0	45·0
−186·5	9·7
−168·0	1·0

The above numbers cannot be considered as more than moderate approximations, but they are sufficient to show the mode of variation of resistance in the two cases.

The values for the resistance of the ice and alcohol have been set out in the two curves in figs. 3 and 4.

These figures show that in the case of the alcohol, as soon as a temperature of −190° pt. is reached the resistance begins to fall with great rapidity or the conductivity to go up.

In the case of the ice the same rapid increase in conductivity begins to take place at about −90°.

We have designed a form of condenser which will enable us to repeat these measurements and free them from some sources of error due to the difference between the contraction of the metal plates of the condenser and that of the dielectric; but pending such more accurate measurements the above figures may be taken as showing approximately the course of events when the ice and frozen alcohol are heated up from the temperature of boiling liquid air.

We add two tables, XI and XII, giving some recent determinations of the dielectric constant of the ethyl alcohol, the results in Table XII showing that evidence apparently exists of abnormal dis-

FIG. 3.

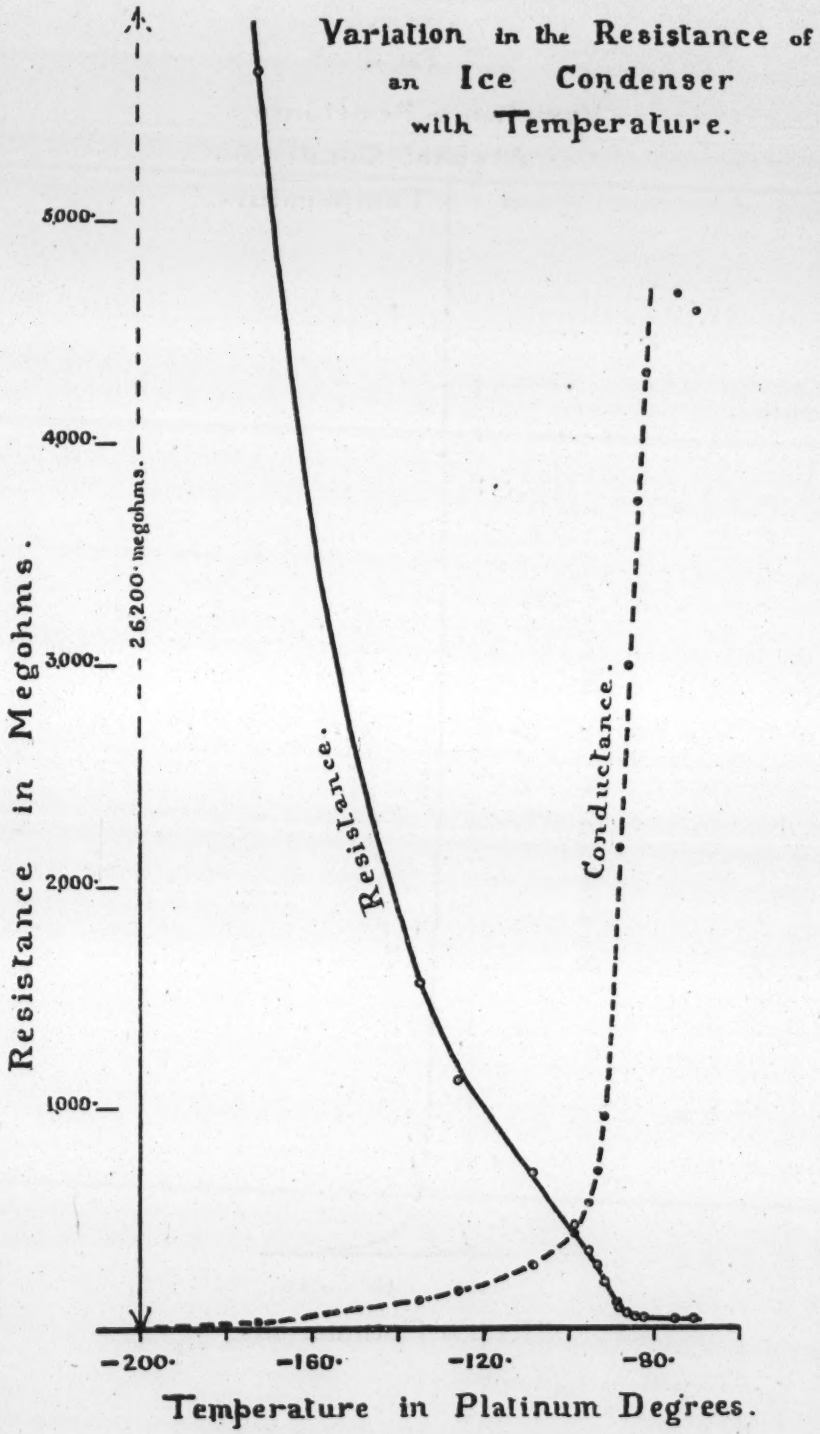
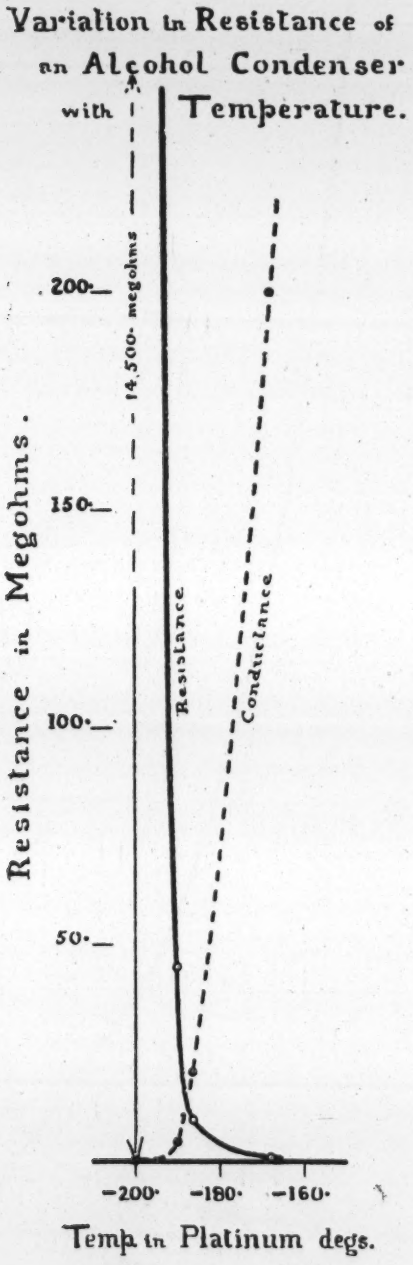

Variation in the Resistance of an Ice Condenser with Temperature.
Resistance in Megohms.
5,000
4,000
3,000
2,000
1,000
26,200 megohms.
Resistance.
Conductance.
-200°
-160°
-120°
-80°
Temperature in Platinum Degrees.

FIG. 4.


Variation in Resistance of an Alcohol Condenser with Temperature.
Resistance in Megohms.
200·
150·
100·
50·
14,500· megohms
Resistance
Conductance
−200·
−180·
−160·
Temp. in Platinum degs.

persion in the case of alcohol for electric radiation lying within the limits of the ten octaves of waves comprised between wave-lengths of 8 mm. and 900 cm. measured in air.*

Table XI.—Determinations of the Dielectric Constant (Specific Inductive Capacity = K) of Ethyl Alcohol by Various Observers.

Observer.	Reference.	Value found per K.	Method.
W. Nernst ...	'Zeits. phys. Chem.,' vol. 14, 1894, p. 622	25·8	By comparing capacities of condensers with telephone.
J. F. Smale ..	'Wied. Ann.,' vol. 57, p. 215	25·8	By deflections of an electrometer filled with alcohol.
E. B. Rosa ...	'Phil. Mag.,' vol. 31, 1891, p. 188	25·7	By attraction between plates of condenser filled with alcohol. Reversals of E.M.F. 2000—4000 per minute.
W. C. Röntgen	'Wied. Ann.,' vol. 52, p. 593	30·5	By alternate currents and condenser (not of such weight perhaps as the others here given).

* The different results found by A. D. Cole for long and short waves have been criticised by J. F. Mohler (see 'Physical Review,' vol. 4, p. 153), who ascribes the differences to conductivity. It has, however, been shown both by E. Cohn and L. Arons ('Ann. der Physik u. Chemie,' vol. 33, p. 13) and by G. U. Yule that the addition of salts to alcohol, which largely affect its conductivity, does not apparently alter perceptibly the dielectric constant, whether determined by slow or rapid reversals of electromotive force.

Table XII.—Determinations of the Electrical Refractive Index (μ) of Ethyl Alcohol for Electric Waves of Various Wave-lengths.

Observer.	Reference.	Value found. μ	Value found. μ^2	Wave-length of waves used in air.	Method.
A. Lampa	'Wien. Sitzungsberichte'.	2·568	6·59	8 mm.	Reduction in length of waves passing into alcohol from air.
A. D. Cole	'Ver. Phys. Ges.', Berlin, vol. 14, 1895, p. 76	3·1 3·2	9·6	5 cm.	Ratio of intensities of original and reflected ray.
P. Drude	'Wied. Ann.', vol. 54, 1895, p. 352	4·74	22·46	60 „	Reduction of wave-length of waves passing into alcohol.
A. D. Cole	'Wied. Ann.', vol. 57, 1896, p. 290	5·24	26·32	259 „	Ratio of intensities of original and reflected rays.
G. U. Yule.....	'Phil. Mag.', vol. 36, p. 531	5·16	26·7	900 „	Reduction in wave-length of waves passing into alcohol.
C. B. Thwing.....	'Zeits. phys. Chem., vol. 14, p. 286, 1894	5·00	25·0	—	Resonance of electric circuits. Frequency not stated, but high.

"On the Relation between Magnetic Stress and Magnetic Deformation in Nickel." By E. TAYLOR JONES, D.Sc. Communicated by Professor ANDREW GRAY, F.R.S. Received January 13,—Read February 25, 1897.

(Abstract.)

The experiments described in the paper constituted an attempt to determine whether the change of length which a nickel wire experiences when placed in a magnetic field can be explained by means of the stresses which are known to exist in consequence of the magnetisation.

The method of calculating the effect of magnetic stresses on the dimensions of a magnetised body was given by Kirchhoff and applied by Cantone to the case of an ellipsoid of revolution of soft magnetic material placed in a uniform longitudinal field.

The expression given by Cantone for the elongation of an ellipsoid of revolution contains terms representing the effect of Maxwell's system of stresses, and also terms representing the effect of those stresses which arise in consequence of the fact that magnetisation depends upon strain.

It is shown that the latter terms reduce, when the eccentricity of the ellipsoid is great, to $\frac{1}{2}H\frac{\delta I}{\delta P}$, where δI is the increase of magnetisation of a cylindrical portion of the material caused by an increase of tension δP per unit area, and H is the magnetising field.

It is also shown by an independent method, due to Professor J. J. Thomson, that the term $\frac{1}{2}H\frac{\delta I}{\delta P}$ represents the elongation of a long cylinder of the material, supposed uniformly magnetised, due to the stresses which exist in consequence of the dependence of magnetisation on strain. Hence, since this term is generally numerically great in comparison with the terms representing the effect of Maxwell's stresses, a very long cylinder of the material could be used for the measurement of both the magnetic elongation and the effect of tension on magnetisation.

The specimen used in the experiments was a long wire of annealed nickel. The magnetisation and the effect of increase of tension on magnetisation were first measured for a series of field-strengths and for two different tensions, these measurements being made by the ballistic method.

Then the magnetic elongation was measured, in independent experiments, for a series of field-strengths and the same two tensions, it being arranged that the nickel wire was in the same magnetic

state in the elongation experiments as in the magnetisation experiments. The change of length of the wire was magnified and measured by causing a lever, whose arms were in a great ratio, to deflect a mirror which was observed by means of a telescope and scale at a considerable distance.

The observed change of length was always a contraction. The change of length calculated from the theory of stresses was also always a contraction, the preponderating term $\frac{1}{2}H\frac{\delta I}{\delta P}$ being always negative.

The difference between the observed and calculated contractions was, however, considerable, the theory only accounting for a part of the observed contraction.

Diagrams are given showing the values of the magnetisation, of the effect of increase of tension on magnetisation, and of the observed and calculated contractions, all as functions of the field H.

It was found that the difference between the observed and calculated contractions was approximately proportional to the fourth power of the magnetisation, a diagram showing that, if this corrected contraction be represented as a function of I^4, the points all lie very near a straight line passing through the origin, the deviations being within the limits of experimental error.

"On the Relations between the Cerebellar and other Centres (namely Cerebral and Spinal) with especial Reference to the Action of Antagonistic Muscles. (Preliminary Account.)"

By Max Löwenthal, M.D. (Würz.), M.R.C.P., and Victor Horsley, F.R.S., F.R.C.S. Received February 8,—Read February 25, 1897.

(From the Laboratory of Chemical Pathology in University College, London.)

The following is a brief summary of certain results which we have obtained in the investigation of the relations prevailing between the cerebellum and other parts of the nervous system, and which we commenced in consequence of an observation made by one of us (L), on the 24th May, 1895.

This consisted in the observation that when both cerebral hemispheres were removed and, as a result, active extension tonus* of the limbs was obtained, excitation (faradic) of the upper surface of the cerebellum caused immediate relaxation of such tonus so long as

* We venture to propose the term "accerebral" tonus for the phenomenon, to avoid unnecessary periphrasis, and to distinguish it from that observed after removal of the cerebellar hemispheres.

the current was applied, and that on the latter being shut off the tonus was immediately re-established.

As this observation appeared to indicate a distinct relation between the cerebellum and the bulbospinal system of lower centres, it was obviously very important to examine it further, especially as to whether it was a truly central effect or not.

It was soon made clear that the effect was a constant one, provided that the above mentioned tonus was distinctly established, and further that the primary effect of excitation of the cerebellum upon the upper limb in such case was not an active relaxation of the triceps alone, but that there was a marked contraction of the biceps.

Further, this effect, though constant, when tonus is present, was not the character of the muscle changes obtained when the tonus was absent.

It was, therefore, necessary to arrange a large series of experiments to determine a comparison between the changes in the two groups of muscles according as to whether the cortex cerebri, corona radiata, crusta, tegmentum, cerebellum, or cerebellar peduncles were excited either singly or in combination.

After making fifteen experiments in which the contraction of the muscles involved was determined by inspection, passive movement and palpation only, the movements produced were graphically recorded, and the general conduct of the experiments arranged as follows :—

Method of Experimentation.

The animal (the dog and cat have so far been examined) was anæsthetised by ether, tracheotomy performed for the purposes of convenient anæsthesia and a ligature looped loosely round each common carotid artery. The left sigmoid gyrus, the right occipital lobe and right half of the cerebellum were freely exposed, the dura mater preserved intact, and the whole kept warm by pads of absorbent wool soaked in warm normal saline solution.

The right forearm was next carefully detached at the elbow after the brachial artery had been ligatured, the nerve trunks cut short, and the lower bony insertions of the biceps and brachialis anticus separated from the ulna and radius shafts. A ligature was then attached to these conjointly, and a similar ligature was fastened to the olecranon, which had been detached by a cut through the bottom of the greater sigmoid notch.

The threads securing the muscles were affixed to Fick's spring myographs of suitable strength, and a record thus obtained on paper of the changes in form of the muscles, as well as of the moment and duration of excitation by suitable signals (F. J. Smith's).

The nerve centres were excited in all cases by fine platinum elec-

trodes, 2 mm. apart. The induction coils used were, one on Kronecker's pattern, the other by the Cambridge Scientific Instrument Company.

History of Previous Observations.

The history of previous investigation of the relations of the cerebellum to the other regions of the central nervous system we intend to discuss in a future communication, which will contain the full details of our experiments.

*Synopsis of Results.**

A. *Excitation of the Left "Motor" Cortex*, i.e., *Forelimb Area of the Sigmoid Gyrus of the Left Hemisphere.*

In the dog and cat excitation of the forelimb area of the excitable cortex produced out of fourteen experiments invariably synchronous contraction of both biceps and triceps muscles. The degree of contraction also was remarkably symmetrical, and in the majority of cases even the relative degree of tonus and clonus respectively was constant.

Further, when quasi-purposive reflex (?) movements occurred during alterations in the anæsthesia, &c., this symmetry was even more pronounced.

So far as antagonism is concerned, when the cortex alone was excited, no relaxation of either the biceps or triceps was observed. It will be remembered that these results are purely those obtained from carnivorous animals.

B. *Excitation of the Left Corona Radiata.*

Stimulation of the corona radiata produced in eight cases invariably symmetrical and synchronous contraction of both biceps and triceps. This was an absolutely constant result in all the cases examined. No relaxation was ever obtained.

C. *Excitation of the Crusta of the Left Crus Cerebri.*

In sixteen experiments the crura were excited on the cut surface. The excision of the cerebral hemispheres was always performed by ligaturing both common carotid arteries, and then dividing the mesencephalon by a single incision passing through, or just in front of, the anterior corpora quadrigemina.

* It is to be remembered that the muscles prepared for graphic record were in all experiments the biceps and brachialis anticus and the triceps, of the *right* side only.

In all the experiments the excitation was performed before the general tonus was established, as well as after in the instances where the tonus actually occurred.

(*a*) *Excitation of the left Crusta before General Tonus is established.*—When the crusta was stimulated in the absence of tonus there resulted invariably contraction of both the biceps and triceps.

(*b*) *Excitation of the left Crusta after General Tonus is established.*—When the "acerebral" tonus is established. Excitation of the left crusta produced out of the sixteen experiments :—

In 13 cases	contraction	of biceps	and	contraction	of	triceps.
,, 2	,,	,,	,,	relaxation	,,
,, 1	,,	relaxation	,,	contraction	,,

In his most recent communication* on the subject of antagonistic muscle action, Sherrington states that he has obtained cessation of the "acerebral" triceps tonic contraction by excitation of the crus pyramidal fibres. As he does not state how often he observed this effect, we cannot compare the conditions of the two sets of experiments.

D. *Excitation of the Crusta of the right Crus Cerebri.*

The right crusta was also excited in eight experiments, and it was found that, as a rule, there was evoked contraction in both biceps and triceps, but twice relaxation of the biceps and once of the triceps was observed.

E. *Excitation of the Tegmentum of the left Crus Cerebri.*

The surface of the tegmentum was excited, and an effect upon the biceps and triceps observed when the stimulation was applied to the region of the red nucleus and the locus niger. Excitation of the anterior corpora quadrigemina was without effect.

In every case in which the left tegmentum was excited it evoked contraction of both biceps and triceps.

F. *Excitation of the Tegmentum of the right Crus Cerebri.*

The right tegmentum, when stimulated in five cases, gave always contraction of both muscles, as a rule, the triceps being in excess of the biceps, but this was reversed on two occasions.

G. *Excitation of the Cerebellum.*

Excitation of the cerebellum produced very constant and sharply marked effects according as to whether the "acerebral" tonus was

* 'Roy. Soc. Proc.,' January 21, 1897.

present or not, and, further, a notable difference according to the side of the cerebellum excited.

The limits of the excitable region of the cerebellum will not now be discussed, but we may here state that the most excitable area is along the line of junction of the vermis superior with the lateral lobe. (This, it will be noted, is that portion of the cortex of the cerebellum which, in vertical position, is nearest the superior cerebellar peduncle.)

For the purposes of the present communication excitation of the cerebellum means excitation of this focal area.

(a) *Excitation of the Cerebellum when "acerebral" Tonus is established.*—The effect of excitation of the cerebellum when ablation of the cerebral hemispheres has produced general tonic contraction, in extension of the trunk and limb muscles is a striking and *constant* active relaxation so far as the triceps (of the side stimulated) is concerned, and an active contraction of the biceps.

In the absence of graphic record it is impossible to determine precisely what is the nature of this effect on either the biceps or triceps.

We have, however, proved by the graphic method that—

(1) The biceps contracts powerfully;

(2) The triceps actively relaxes, *i.e.*, it does not simply relax by ceasing to contract, but it relaxes so as to lose its normal physiological tonus.

The next step was of course the further investigation whether this phenomenon is unilateral. We have found as the result of twenty-nine experiments that the muscles of the right arm can be influenced by excitation of both halves of the cerebellum, but that the effect is very much greater from excitation of the right half, *i.e.*, the same side. In addition we have excited the two halves of the cerebellum after median division of the organ, and then found that the effect was practically limited to the same side.

This is of course in harmony with the results of ablation of the cerebellar hemispheres obtained by Luciani, Risien Russell, and Ferrier and Turner.

In five experiments it was interesting to find that excitation of the *left* half of the cerebellum, *i.e.*, that of the opposite side, evoked relaxation of the *biceps*. This crossed effect we noticed early in the investigation, and the similarity in the movements of the forelimb in walking was very striking.

The phenomenon of relaxation of the triceps and contraction of the biceps can be obtained also by excitation of the sub-cortical white substance of the cerebellum and of the peduncles in part.

(b) *Excitation of the Cerebellum when no "acerebral" Tonus is yet*

established.—We have constantly obtained from the cerebellar cortex a tonic *contraction* of either the triceps or biceps, or both together, if the "acerebral" tonus had not appeared.

H. *Synchronous Excitation of the Cerebellum and Cerebrum.*

Synchronous excitation of the cerebral area for the forelimb and the cerebellar focus has given so far, in cases where the cerebellum is definitely excitable, an addition to the tonus elicited previously from the cortex cerebri.

While such reinforcement seems to be the rule, we believe that the clonic *character* of the cortical impulses is adversely affected by the cerebellar activity.

Since Professor Sherrington's third note on "Reciprocal Innervation of Antagonistic Muscles" appeared, we have investigated the question of *reflex* inhibition of the "acerebral" tonus by the graphic method, to see how far the two effects are identical, and have obtained some instructive results, the full discussion of which, however, must be postponed, as the observations are incomplete. It is sufficient now to say that while we have confirmed Professor Sherrington's discovery that excitation of an afferent tract on the same (*i.e.*, in our experiments, *right*) side causes relaxation of the triceps and contraction of the biceps, we have also found that excitation of a *left* afferent tract causes relaxation of the biceps and contraction of the triceps, an interesting correspondence with the alternation we observed in the two halves of the cerebellum, and which is obviously related to the movement of progression.

"On the Action of Light on Diastase and its Biological Significance." By J. Reynolds Green, Sc.D., F.R.S., Professor of Botany to the Pharmaceutical Society of Great Britain. Received January 28,—Read February 25, 1897.

(Abstract.)

According to the observations of Brown and Morris,* the quantity of diastase in foliage leaves undergoes considerable variations during the twenty-four hours of the day, being greatest in the early morning and least in the evening, particularly after several hours' sunshine. During the past three years the author has carried out a series of experiments to ascertain whether the diminution in quantity is due to a destructive influence of the light upon the enzyme, similar to

* 'Jour. Chem. Soc.,' May, 1893.

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that which several observers, especially Marshall Ward,* have noticed it to have upon the life of micro-organisms and other lowly forms of vegetation.

The method of investigation has been to expose various solutions containing diastase to the action of light for several hours, either that of the total spectrum or of selected portions of it, and after such exposure to test the hydrolysing power of the solutions upon a weak solution of soluble starch or upon a 1 per cent. starch paste. Controls have been carefully kept during each experiment, and so arranged that any difference shown in diastatic power has been clearly attributable to the illumination.

The diastatic solutions have been extract of malt; solution of diastase precipitated from such extract by alcohol; diluted human saliva freed from mucin; and extracts of foliage leaves. The solutions have been preserved by the addition of 0·2 per cent. of potassic cyanide.

The source of illumination has been either bright sunshine, diffused daylight, or the light from a powerful naked electric arc, access to the latter having been obtained through the kindness of Mr. Barker, the manager of the electric light works at Cambridge.

The diastatic power has been ascertained by titrating the results of the digestions with Fehling's fluid, combusting the filters with the precipitates, and weighing the resulting cupric oxide.

When all the rays of the spectrum were under investigation, the diastatic extracts were exposed to the light source either in quartz-fronted cells, or mixed with films of agar-agar, the former method giving the most satisfactory results.

From many repeated experiments it was found that exposure to the whole of the spectrum for several hours caused the destruction of from 20 to 60 per cent. of the diastase.

When the ultra-violet rays were cut off by the use of glass vessels, glass having been proved by many observers to be opaque to these rays, the effect was different. At first and for some time there was a very considerable increase in the diastase, but this was succeeded on a prolonged exposure, lasting several days, by a gradual and almost complete destruction of the enzyme.

By the use of a series of screens, arranged according to the directions of Landolt,† the visible spectrum was divided into five regions: *red*, extending from wave-length 720 $\mu\mu$ to 640 $\mu\mu$; *orange*, 640 $\mu\mu$ to 585 $\mu\mu$; *green*, 585 $\mu\mu$ to 500 $\mu\mu$; *blue*, 500 $\mu\mu$ to 430 $\mu\mu$; and *violet* beyond 430 $\mu\mu$, and these several bands were examined separately.

Details of the experiments are given in the complete paper. The results may be stated in percentages.

* 'Phil. Trans.,' B, vol. 185, 1895.

† 'Ber. d. Deut. Chem. Gesell.,' 1894, p. 2892.

The infra-red, red, orange, and blue regions gave increases of 10·8, 53·5, 4·75, and 20·8 per cent.; the green a diminution of 15·7 per cent. The violet gave also a marked diminution, but it was not measured, as no satisfactory screen, allowing only the violet rays to pass, could be devised. The paper is illustrated by a curve showing these effects.

The effects of the illumination were found to be progressive, the increase or diminution of the diastase continuing after the solutions were removed from the access of the several rays.

The deleterious rays were found to be absorbed by the solution, the absorption being effected partly by the diastase itself, whether boiled or unboiled, and partly by the proteids present in the extracts. After removal of the latter by boiling and filtration, the solution was still opaque to the rays.

The screening influence of proteids was examined separately, by adding small quantities of egg-albumin to the extracts. The latter was found to be protective, the degree of protection being however, only roughly proportional to the amount of albumin present.

The colouring matter of the barley grain was also ascertained to act as a screen for the deleterious rays.

The living leaf was also examined by the same method as the extracts, and the diastase in it was found to undergo a similar destruction under the influence of the light.

It was found impossible to investigate the possible protective influence of chlorophyll in the leaf, as all the solvents of the latter proved to be opaque to the ultra-violet rays. They confirmed, however, the view that the violet rays, and possibly the green, have a destructive effect upon the enzyme.

The experiments lead to the conclusion that there exists in the leaf and in the various extracts examined a certain amount of zymogen which is converted by the infra-red and the red, orange, and blue rays into active diastase. This conclusion is supported by experiments, detailed in the paper, upon the effect of keeping the solutions for several days at a temperature of 38° C. A curve of the effect of this exposure is given in the paper.

The violet and ultra-violet rays, on the other hand, cause a destruction of the diastase, or at least such a change in the configuration of its molecule that it is unable to effect the hydrolysis of starch.

Three other conclusions of some importance have resulted from the experiments:—

1. That the enzyme is not located in the chlorophyll grain, but in the protoplasm of the cell.

2. That the suggestion of Pick* and Johow† that the red colour-

* 'Bot. Central.,' vol. 16, pp. 9—12.

† 'Pringsheim's Jahrb.,' vol. 15, p. 299.

ing of certain leaves is a material help to the translocation of starch in them is probably sound, as such colouring matters screen off the rays which destroy the diastase.

3. That there exists in plants a power of absorbing and utilising the radiant energy of light, sometimes to a considerable extent, without the presence of a chlorophyll apparatus.

The last conclusion appears to be of very far-reaching importance, supplementing other observations already published by Engelmann,* Winogradski,† Speschnew, and other observers, none of whom, however, have indicated such utilisation of the rays of the visible spectrum.

"Fragmentation in *Lineus gesserensis*." By ALEX. BROWN, M.B., B.Sc., M.A., Lecturer in Zoology, and Senior Assistant in the Natural History Department, University of Aberdeen. Communicated by Professor MCINTOSH, F.R.S. Received February 1,—Read February 25, 1897.

(Abstract.)

The following results are deducible from the investigations described in the paper:—

1. The zones of fission in *Lineus gesserensis* coincide with the transverse markings observable externally at definite intervals on its body.

2. The process of fission in all cases proceeds from *within outwards*, *i.e.*, from the digestive canal towards the cutaneous tissues.

3. The process of fission is a process of solution, and that, too, through the agency of the digestive fluids of the alimentary tract.

4. The solution of the tissues of the body-wall at the zones of fission is preceded in those regions by circular outgrowths of intestinal epithelium together with the formation of corresponding external grooves.

5. In the plane passing through any zone of fission, there is distinct evidence of the exertion of continuous pressure in opposite directions, or pressure exerted outwards through the intestinal outgrowth is met by pressure exerted inwards in the same plane through the body-wall.

6. As the result of these opposing forces, atrophy, disintegration and disappearance of the outermost cells of the intestinal outgrowth take place. Thus the layers of the body-wall are brought into con-

* 'Bot. Zeit.,' 1888.

† 'Ann. de l'Institut Pasteur,' 1890—1891.

tact with the digestive cavity, and solution of the tissues proceeds outwards.

7. The ruptured surfaces are at once covered up by the intestinal outgrowth, and by the proliferation of the subjacent connective tissue.

8. The connective tissues all along the zone of fission multiply in order to aid in the healing of the fragments.

9. The intestinal epithelium changes in character as it takes up its new external position.

10. The whole process of fission has evident relationships to the process of sexual reproduction.

March 4, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read, as follows:—

Allen, Alfred Henry, F.C.S.
Baker, H. Brereton, M.A.
Barrett, Professor W. F., F.R.S.E.
Beevor, Charles Edward, M.D.
Bell, Robert, LL.D.
Benham, William Blaxland, D.Sc.
Binnie, Alexander Richardson, Mem. Inst. C.E.
Broadbent, Sir William Henry, Bart.
Brown, Professor Ernest William.
Budge, Ernest A. Wallis, D.Litt.
Burch, George James, M.A.
Callaway, Charles, D.Sc.
Cardew, Philip, Major, R.E.
Chree, Charles, M.A.
Corfield, William Henry, M.D.
Crookshank, Professor Edgar March, M.B.
Dixon, Professor Alfred Cardew, M.A.
Dresser, Henry Eales.
Edgeworth, Francis Ysidro, M.A.
Elwes, Henry John, F.L.S.
Etheridge, Robert, F.G.S.
Gamble, James Sykes, M.A.
Gray, Professor Thomas, B.Sc.
Haddon, Alfred Cort, M.A.
Haldane, John Scott, M.D.
Harcourt, Leveson Francis Vernon, M.Inst.C.E.
Harmer, Sidney Frederic, M.A.
Haswell, Professor William A., D.Sc.
Head, Henry, M.D.
Howes, Professor George Bond, F.L.S.
Kanthack, A. A., M.D.
Kipping, F. Stanley, D.Sc.
Lansdell, Rev. Henry, D.D.
Lewes, Professor Vivian B., F.C.S.

Lewis, W. Bevan, F.R.C.S.
McConnell, James F. P., Surgeon-Major.
McMahon, Lieutenant-General Charles Alexander.
Mallock, Henry Reginald Arnulph.
Mansergh, James, M.Inst.C.E.
Martin, John Biddulph, M.A.
Mathews, George Ballard, M.A.
Matthey, Edward, F.C.S.
Mill, Hugh Robert, D.Sc.
Morgan, Professor Conroy Lloyd, F.G.S.
Muir, Thomas, M.A.
Murray, George Robert Milne, F.L.S.
Neville, Francis Henry, M.A.
Nicholson, Professor H. Alleyne, M.D.
Notter, James Lane, Surgeon-Lieut.-Col.
Oliver, John Ryder, Major-Gen., R.A.
Preston, Professor Thomas, M.A.
Rambaut, Professor Arthur A., M.A.
Reid, Clement, F.G.S.
Reid, Professor Edward Waymouth, M.B.
Salomons, Sir David, M.A.
Scott, Alexander, M.A.
Seward, Albert Charles, M.A.
Shenstone, William Ashwell, F.I.C.
Smith, Professor William Robert, M.D.
Tanner, Professor Henry William Lloyd, M.A.
Tatham, John F. W., M.R.C.P.
Taylor, Henry Martyn.
Thomson, Professor John Millar, F.C.S.
Trouton, Professor Frederick Thomas, M.A.
Turner, Professor Herbert Hall, M.A.
Walker, Professor James, D.Sc.
Waterhouse, James, Colonel.
Whymper, Edward, F.R.G.S.
Wimshurst, James.
Windle, Bertram Coghill Allen, M.D.
Woodhead, German Sims, M.D.

Pursuant to notice, M. E. Amagat, Professor F. Cohn, Professor J. Willard Gibbs, Professor R. P. H. Heidenhain, and Dr. R. Koch were balloted for and elected Foreign Members of the Society.

The following Papers were read:—

I. "Experiments on the Absence of Mechanical Connection betwen Ether and Matter." By Oliver Lodge, D.Sc., F.R.S., Professor of Physics, University College, Liverpool.

II. "Second Report on a Series of Specimens of the Deposits of the Nile Delta, obtained by Boring Operations undertaken by the Royal Society." By John W. Judd, C.B., LL.D., F.R.S., Professor of Geology in the Royal College of Science.

III. "The Palæolithic Deposits at Hitchin and their Relation to the Glacial Epoch." By Clement Reid., F.L.S., F.G.S., of the Geological Survey of the United Kingdom. Communicated by Sir Archibald Geikie, F.R.S.

IV. "Luminosity and Photometry." By JOHN BERRY HAYCRAFT, M.D., University College, Cardiff. Communicated by Professor SCHÄFER, F.R.S.

"Experiments on the Absence of Mechanical Connection between Ether and Matter." By OLIVER LODGE, D.Sc., F.R.S., Professor of Physics, University College, Liverpool. Received January 19,—Read March 4, 1897.

(Abstract.)

The author gives an abbreviated account of a long series of experiments conducted by himself and his assistant, Mr. Davies, in continuation of those related in 'Phil. Trans.,' A, 1893 (Aberration Problems, &c.). The method consists in bifurcating a beam of light, and sending each half in opposite directions round a closed periphery very near a rapidly rotating mass of matter, and then observing by means of interference fringes whether the velocity of light is affected in the slightest degree by this neighbourhood of moving matter. The steel disks have been now whirled to higher speeds, chiefly at 3000 revolutions a minute; the steadiness of the machine and the definition of the bands have been improved, other minor improvements have been made, and a long series of micrometric readings have been taken, both at increasing and at decreasing speeds.

Further, the steel disks have been replaced by a much more massive lump of iron, weighing $\frac{3}{4}$ ton, with a narrower channel for the light to travel in; and the bands have been observed close up to the moving surface, and even when reflected in it. The rotation was also continued for some hours to see if by chance *time* had any influence.

Moreover, the iron mass was strongly magnetised by a steady current, so that the light travelled across a moving magnetic field; and lastly the steel disks were replaced, with an insulated third disk between them, and strongly electrified, so that the beam of light travelled across a moving electrostatic field. After a number of spurious disturbances had been gradually eliminated, the author finds that in none of these ways is the velocity of light at all appreciably affected, and accordingly concludes that there is no viscous connexion between the ether and matter of observable magnitude; *i.e.*, that whatever motion moving matter may confer upon the ether must be of an irrotational kind. It was demonstrated theoretically in the previous memoir that no optical experiments could be competent to detect motion of this latter character, and accordingly no attempt has

been made to look for any kind of motion except such as would be caused by something akin to viscosity.

Incidentally the author points out that by rotating the whole optical apparatus and observer, instead of the disks, at a very moderate speed, a shift of the bands should be seen; and even that the earth's rotation would with a large enough frame produce an effect, which latter, however, it appears difficult or impossible to observe, not on account of its smallness, but on account of its constancy.

The effect to be expected on Fresnel-Fizeau principles from whirling *air*, was unfortunately just too small for the author to safely observe. The residual disturbing causes just masked it, but it is probably not beyond the reach of another attempt with a still more thoroughly steady machine, if anyone feels inclined to persevere so far. At the same time if it be supposed that any microscopic trace of true ether effect still possibly exists (which the author wholly disbelieves), and if a further attempt be hereafter made to observe it, a number of slight residual disturbing causes would be got rid of (and probably other difficulties introduced) by rotating the machine in a vacuum.

"Second Report on a Series of Specimens of the Deposits of the Nile Delta, obtained by Boring Operations undertaken by the Royal Society." By JOHN W. JUDD, C.B., LL.D., F.R.S., Professor of Geology in the Royal College, of Science. Communicated by desire of the Delta Committee. Received February 11,—Read March 4, 1897.

The last report on the borings undertaken in the delta of the Nile under the auspices of the Royal Society was communicated to the Society by the direction of the Delta Committee on November 12, 1885, and published in No. 240 of the 'Proceedings.' This report dealt with the materials obtained from the three borings made at Kasr-el-Nil, at Kafr-ez-Zayat, and at Tantah, which reached depths of 45 feet, 84 feet, and 73 feet respectively. Although these borings made known to us the character of the delta deposits at greater depths than the explorations made by Mr. Leonard Horner and M. Linant de Bellefondes, yet none of them succeeded in reaching the solid rock on which these deposits lie, and in which the Nile Valley was originally excavated. It was therefore decided by the Delta Committee to make still more strenuous efforts to attain this result—a result which the sections published by Figari Bey, said to be based on borings made for the purpose, led the Committee to believe might be arrived at with a moderate expenditure.

In their attempts to carry out this important work, the Delta Com-

mittee have received the most valuable aid from the Secretary of State for War, the Inspector-General of Fortifications, and the officers of the detachment of the Royal Engineers attached to the Army of Occupation in Egypt; and their thanks are especially due to Colonel Turner, R.E., Captain Dickenson, R.E., Lieutenant Godly, R.E., and Corporal Bellinger, R.E. To the Egyptian Railway Administration the Committee is indebted for permission to choose a site for boring on their land, and for much assistance given from time to time.

Zagazig having been chosen by the Royal Society Committee as a suitable site for the next attempt to penetrate the delta deposits, a Legrand-and-Sutcliffe boring apparatus, which had already been set up and tested at Kasr-el-Nil, was brought to the selected locality, and arrangements were made for carrying the boring to the depth of 100 feet with a 5-inch lining tube, and to another 100 feet with a 4-inch tube. It was considered certain at that time—and many published statements supported the belief—that the solid rock of the Nile Valley could not fail to be met with at a depth of *less than* 200 feet. The spot where the boring commenced had an elevation of 26 feet 1 inch above the sea-level at Alexandria, as determined by the Egyptian Public Works Department.

A pit having been dug to the depth of 8 feet, and a platform erected over it, the 5-inch pipes were driven in and carried without any great difficulty to a depth of 97 feet from the surface. At this depth a 4-inch pipe was driven within the other, and the boring operations were proceeded with. When, however, a depth of 190 feet 6 inches had been reached, it was found impossible to drive the pipe farther, its bottom being still in a quicksand. Thus the work, which had been commenced on May 7, 1886, had to be discontinued on August 14 of the same year.

On the recommendation of Captain Dickenson, R.E., who had so ably and successfully directed these operations, it was decided by the Delta Committee to resume the work at this borehole in the following year, with a 3-inch pipe. The necessary apparatus having been sent out, the work was resumed on April 21, 1887, and by vigorous and skilful efforts carried to a depth of 339 feet 6 inches.

At that point, however, it was found impossible to drive the 3-inch pipe farther, but a rod was pushed down 5 feet 6 inches without reaching solid rock: the exploration thus attained a total depth from the surface of 345 feet, or 319 feet below the sea-level.

From the surface to a depth of 115 feet the strata passed through in the Zagazig boring closely resembled those already reported upon as occurring in the three earlier borings of Kasr-el-Nil, Kafr-ez-Zayat and Tantah, and consisted of alternations of desert-sand and Nile-mud. All the samples sent home have been carefully

examined, and the minerals in them are found to be identical in character with those in the deposits already described. Some small pieces of pottery and bone were found at depths varying from 8 to 27 feet from the surface.

In the alluvial mud, which prevails from the surface to the depth of 20 feet, small tubular and knot-like bodies abound. These are undoubtedly formed by the deposition of calcium carbonate on the rootlets of plants, giving rise to beautiful calcareous tubes, and to knot-like bodies where the rootlets branch. These calcareous tubes and knot-like bodies are covered on the outside by cemented mud-grains, and present a singular resemblance to some of the arenaceous foraminifera like *Astrorhiza*. The knot-like bodies are evidently formed where the rootlets have branched or become twisted, and certain of them simulate in outward form some of the bodies referred to as *Stacheiæ*. As we go downwards in the deposit, these bodies become fewer in number and less definite in form; and in the clay from depths between 75 and 92 feet, we find no such tubular bodies, but instead of them masses of concretionary limestone or "race," probably resulting from the solution and re-deposition of the calcium carbonate. The sand, which prevails from depths of 20 to 75 feet, varies in coarseness, and is almost always well rounded and evidently wind-borne, and is in places indurated into a sand-rock or imperfect sandstone.

From 75 feet to 92 feet an indurated alluvial mud was found. The tube-like bodies are here wanting, but in their place we find the irregular masses of concretionary argillaceous limestone or "race," evidently formed, as we have seen, by the solution and re-deposition of the calcium carbonate originally deposited on the root-fibres. The most careful washing of these clays by my assistant, Mr. F. Chapman, failed to reveal any trace of contemporary organisms, but at a depth of 90 feet he obtained a good specimen of a Nummulite (*N. Guettardi*, d'Arch. and Haime) evidently derived from the Eocene limestones of the country.

At the depth of 115 feet a very noteworthy change was found to occur in the characters of the beds passed through, a mass of coarse sand and shingle being met with, and this continued to the depth of 151 feet. At the latter depth a band of yellow clay 2 feet thick was passed through, and under it sand and shingle beds prevailed till the lowest depth reached, 345 feet. Specially coarse shingle beds were found at depths of 121 feet, 160 feet, 175 feet, 190 feet, 208 feet, 250 feet, 265 feet, and 270 feet. In some of these shingle beds the fragments, which were usually well rounded—often, indeed, perfect pebbles—were very coarse, the fragments being of all sizes up to that of a hen's egg.

It is interesting to note that a boring made at Rosetta in the

summer of 1885 by Mr. T. E. Cornish, C.M.G., Director of the Alexandria Waterworks, gave a section very similar to that at Zagazig. The object of this boring was to obtain a supply of fresh water for the town of Rosetta, but success not having been attained when a depth of 153 feet 4 inches from the surface had been reached, it was found too costly to carry the work farther.

For details of this interesting boring the Royal Society's Committee is indebted to Sir Colin Scott Moncrieff, R.E., K.C.M.G., at that time Under-Secretary of State for the Public Works Department of the Egyptian Government. The boring was carried down by a 5-inch tube, the surface being 9 feet 4 inches above mean sea level. Various beds of sand and mud, the latter containing in some places impure lignite, were found down to the depth of 143 feet 8 inches from the surface, but at this latter depth a mass of "coarse sand and pebbles" was found, which was followed down for about 10 feet. No specimens from the boring have reached this country.

It will thus be seen that in the case of the Zagazig boring we find at the depth of 115 feet 8 inches (89 feet below sea level) a sudden change from the blown sand and alluvial mud of the Nile delta to masses of shingle and sand, and that the same change is found to take place at the Rosetta boring at a depth of 143 feet 8 inches (134 feet 4 inches below sea level). That these shingle beds were deposited under totally different conditions to those which prevailed while the delta deposits were laid down, and that they were in fact the product of ordinary fluviatile action, can scarcely be doubted, and the determination of the geological age of the great gravelly deposit which is now shown to underlie the modern delta deposits, and to attain depths which certainly in places exceed 230 feet, becomes a problem of the greatest importance and interest.

That the surface of these old gravelly deposits is a very uneven one is indicated by the difference of depth at which it is found at Rosetta and Zagazig respectively. It is possible, indeed, that this gravelly floor may in places rise through the whole of the Nile deposits, and form the present surface of the country. The late Sir Samuel Baker, in a letter addressed to the Royal Society's Delta Committee on February 20, 1886, called attention to the existence of the so-called "turtle-backs," which he regards as interesting proofs of "the pre-existence of desert, before the Nile deposit had converted the lower level into delta."

"There are," he says, "several of these turtle-backs, varying from two to twenty acres, which represent the original desert that has, from its superior level, never been subjected to the inundation of the river. These patches of sand appear like islands in the wide expanse of dark alluvium, which exhibits the maximum deposits of the Nile." Several of these turtle-backs may be seen from the rail-

way between Tantah and Cairo. A sketch section, given by Sir Samuel Baker, shows that in his view they form part of the original floor on which the Delta deposits were laid down.

In considering this suggestion of Sir Samuel Baker, however, it should be borne in mind that the alluvial mud of the Nile alternates with considerable masses of blown sand, and that the turtle-backs may be the surfaces of great lenticular patches of such blown sand. Communications which have been made to me by Colonel J. C. Ross, R.E., C.M.G., formerly Inspector-General of Irrigation, seem to point to the same conclusions as those arrived at by the late Sir Samuel Baker. Colonel Ross states that the turtle-backs sometimes show a distinctly shingly character like that of the materials derived from the borings.

Bearing in mind the extreme importance of determining the geological age of the deposits underlying the delta-deposits, I have made the greatest efforts to obtain contemporary fossils from their materials. The clays have been most carefully washed and picked over, and the sands sifted and searched; but although derived fossils are in some cases very abundant, not a single organism has been found which lived when the beds were deposited, and would serve to throw light on the geological period to which they must be assigned. Even the fine clays between 151 and 153 feet, which, it was hoped, might have permitted of the preservation of some organic forms, have proved to be hopelessly barren of fossil remains.

As it was of considerable interest to determine the source of the various pebbles making up the shingle deposit, which we may conveniently speak of as the "Sub-delta formation," I placed myself in communication with Dr. Karl von Zittel, F.M.G.S., of Munich, who possesses such a unique knowledge of the rocks and fossils of North Eastern Africa. In his obliging communication, he has indicated the probable source of the pebbles which I forwarded to him, and writes as follows :—

"The quartz and chalcedony pebbles from depths of 120, 160, 245, and 270 feet are almost absolutely pure examples of those rocks. The sandstones (for example, those from the depth of 120 feet) rather recall, in their general appearance, the Tertiary Sandstone of Gebel Achmar, near Cairo, than the older (Cretaceous) Nubian Sandstone of Upper Egypt. The quartz and chalcedony pebbles, before referred to, might also be derived from the Gebel Achmar Sandstone. The absence of limestone pebbles is striking; it would appear that only the harder rocks have been preserved in the gravels of the Delta, the softer ones having been possibly worn away."

With respect to the igneous rocks found as pebbles in these shingle beds, Dr. von Zittel suggests, from their macroscopic appearance, that they may be derived "from the side valleys of the Arabian Desert."

The microscopic examinations of these pebbles of igneous rocks shows that they consist of much altered hornblende andesite, or porphyrite (208 feet), and andesitic (porphyritic) tuffs (121 feet, 175 feet, 208 feet). These rocks remind one of those of Djebal Dockran, except from the circumstance that they are not coloured by the manganese epidote like the celebrated "Porfido rosso antico." Altered quartz-andesite, dacite, or quartz-porphyrite (160 feet) with rhyolite, showing beautiful flow-structure and many foreign fragments caught up in its mass (160 feet),—and rhyolite or quartz-andesite tuffs (170 feet, 250 feet) were also found.

Of metamorphic rocks, quartzites, which are evidently altered sandstones, were found somewhat frequently. And in some cases (208 feet, 250 feet) the pebbles showed evidence that the rocks from which they were derived had undergone great crushing—the mylonitic (cataclastic) structure and the brecciated and veined structures affording striking evidence of the deformation to which the rocks had been subjected.

Many pebbles of sandstone, sometimes showing stratification and fault-structures, but destitute of organic remains, were obtained at various depths. Some examples of sand-grains bound together with iron oxide (like our "carstone") also occur among the pebbles. The chief fossiliferous rocks found were as follows:—

Pebbles of flinty limestone from depths of 160, 175, 250 feet contain recognisable Foraminifera, and one pebble, from 175 feet, is crowded with specimens recognised by von Zittel as "belonging to the Textularidæ, Rotalidæ, and Globigerinæ." Of these there are other specimens from a depth of 270 feet. Dr. von Zittel states: "I hold it as probable that these pebbles come from the Eocene of the Nile valley. Flinty layers and concretions are extremely common in the Egyptian Eocene (as, for example, in Central and Upper Egypt). The absence of sections of *Nummulites* and *Alveolina* is particularly noticeable."

At the request of Dr. von Zittel the sections containing Foraminifera were submitted to Professor T. Rupert Jones, F.R.S., who, with the aid of Mr. F. Chapman, has examined and reported upon them. The following notes on certain of the fossiliferous pebbles are based on the reports kindly furnished to me:—

From a depth of 160 feet a pebble was obtained containing both Foraminifera and, apparently, sponge-spicules. The species recognised were—

Globigerina bulloides d'Orb. (a form with a thick prickly test, probably a deep-sea variety).

G. sp., a depressed form.
Cristellaria or *Nummulites?*

A flint with numerous and very well preserved examples of Foraminifera was obtained from a depth of 175 feet, and in several sections made from this pebble the following forms were detected:—

Textularia globulosa, Ehrb.
„ *sagittula*, Defrance.
Bolivina (?) dilatata, Reuss.
„ *punctata*, d'Orb.
Globigerina bulloides, d'Orb.
Discorbina globularis, d'Orb.
Rotalia ammoniformis, Lamk.
Anomalina, sp.
Spiroloculina, sp.

From the same depth of 175 feet a pebble of chert (apparently a silicified dolomitic limestone) yielded the following:—

Bolivina obsoleta, Ehrb.
Globigerina bulloides, d'Orb.

From a depth of 250 feet a flint pebble was obtained, which has all the appearance under the microscope of being a silicified ooze, crowded with specimens of *Globigerina bulloides* d'Orb. (a variety with a thick-walled test), and containing also the following species:—

Rotalia ammoniformis, Lamk.
Polymorphina compressa, d'Orb.
Pulvinulina, sp., &c.

From a depth of 270 feet several flint pebbles were obtained which contained organisms, but these appear to have been partially dissolved before silicification took place, and hence identification is difficult. They include *Globigerina*, *Discorbina*, &c.

The whole of these fossils are regarded by Professor Rupert Jones as proving that the fragments containing them were derived from the Eocene (Nummulitic) limestones of Egypt, thus fully confirming the conclusions of Dr. von Zittel.

At the depth of 121 feet a pebble of a somewhat different class of rock was obtained. It appears to be an indurated sandstone with detrital fragments of wood, Foraminifera of the following species:—

Lagena lævis, Mont.
„ *globosa*, Mont.
Globigerina, sp.
Discorbina, sp.

and, in addition, a Lithistid sponge-spicule, an ostracod valve (Cythere ?), fragments of spines of Echini, a Radiolarian (?), and

oolitic grains (silicified) were detected. This rock may have come from a Cretaceous deposit, and not from the Eocene like the others.

Of the general sources from which these pebbles were derived Dr. von Zittel writes as follows :—

"On the whole it appears to me conceivable that these gravels under the delta originated at a time when the Nile had already formed its present valley, but not to so great a depth as at present. The majority of the rolled rock-fragments would seem not to have been derived from points extremely distant from those in which they are at present found."

In considering the nature and sources of the pebbles found in the boring at Zagazig, it may be well to point out that the spot where the boring was carried out is directly opposite to the Great Wady (W. Tumilat), which opens on the delta from the east, and that much of the materials composing the gravels may have been brought down by this tributary rather than by the main stream of the Nile itself. Hence we may not have in this particular section so good an average sample of the contents of the Sub-delta formation as would be obtained at other localities.

There can scarcely be the smallest doubt that in this Sub-delta formation we have a series of deposits, which were formed under totally different conditions from those which prevail in North Eastern Africa at the present time. The land must have been at an elevation at least from 100 to 300 feet higher than at present, and the Lower Nile, instead of forming an alluvial flat, as at present, must have deposited coarse sands and gravels. It is upon the very uneven surface of this Sub-delta deposit that the alluvial mud and sands of the delta have been deposited, as the surface gradually subsided below the level of the Mediterranean.

The interesting problem of the geological age of this Sub-delta deposit remains to be solved, but it may be hoped that the explorations now being carried on by the Geological Survey of Egypt, under Captain H. G. Lyons, R.E., F.G.S., may furnish new and important evidence bearing on this important question.

It is to be regretted that the borings carried out by the Royal Society have not set at rest the doubts which have long existed as to the depth at which the solid rock-floor lies below the surface of the delta. But while this has not yet been accomplished, it is satisfactory to have been able to show that the supposed insignificant thickness of the alluvial deposits is altogether a mistake, while the existence of an underlying formation, laid down under conditions totally different to those which prevail at present, has been demonstrated.

Communications have lately passed between the English War Department, the Egyptian Public Works Department, and the Royal

Society, which lead us to hope that borings, to be shortly undertaken for economic purposes, may, either with or without aid from this Society, supply the means of reaching greater depths than that attained at Zagazig, and possibly of reaching the old floor of solid rocks on which the Sub-delta deposits rest.

"The Palæolithic Deposits at Hitchin and their Relation to the Glacial Epoch." By CLEMENT REID, F.L.S., F.G.S., of the Geological Survey of the United Kingdom. Communicated by Sir ARCHIBALD GEIKIE, F.R.S. Received February 15,—Read March 4, 1897.

Certain excavations and borings at Hoxne, undertaken in the year 1896 at the cost of the British Association and of the Royal Society, threw much light on the relation of Palæolithic man to the Glacial Epoch. It was thought advisable therefore to examine the similar deposits at Hitchin, to ascertain to what extent the conclusions already arrived at were supported by exploration at a fresh locality. It was desired also to see whether a new locality would aid us in restoring pages in the geological history missing in the Hoxne record. At the instance of Sir Archibald Geikie, a grant of £50 was made by the Council of the Royal Society towards the cost of the necessary excavations, borings, and incidental expenses, unavoidable if the inquiry was to be carried out satisfactorily. Of this sum, only about £30 has been expended, for, after reaching a certain stage with good results, it was discovered that any further advance meant a far greater expenditure of time and money than seemed justified. Work was therefore stopped as soon as the main point under dispute had been cleared up, and a sufficient series of fossil plants had been obtained to determine the climatic conditions that held while the ancient alluvial strata were being deposited.

It is perhaps scarcely necessary under the circumstances to do more than allude to the results of previous work. Palæolithic implements have long been known from Hitchin, and their position in and at the base of a stony brickearth was well ascertained. It was also known that this brickearth rested on loam and shell marl, with fresh-water mollusca and mammalian remains. All this had been made perfectly clear, principally by the researches of Prestwich, Sir John Evans, Mr. W. Ransom, and Mr. William Hill. The most important of the doubtful points were the relations of these ancient alluvia to the widespread sheet of chalky boulder clay, and to the valleys of the existing streams.

In the prosecution of these supplementary researches I have been greatly aided by the local assistance freely rendered by Mr. William

Hill and Mr. W. Ransom. I have also to thank the different landowners and tenants, especially Mr. Theodore Ransom and Mr. Jeeves, for the liberality with which they have permitted excavations and borings to be made on their land.

A preliminary examination of the neighbourhood of Hitchin convinced me that the Palæolithic and ancient alluvial deposits there found occupied a small irregular valley, which had become silted up and almost obliterated. It was desirable to construct an accurate geological section across this valley, such as we were able to draw at Hoxne; but it seemed likely that conditions would prove much less favourable than at Hoxne, and that the coarse, loose, and watery character of the strata would render work difficult. Such proved to be the case, and, instead of being able to make a connected chain of borings, it was necessary to select sites where the difficulties could best be avoided. The result of this method of work has been, that while the general structure and relation of the deposit has become perfectly clear, it is impossible to draw an accurate section across the old valley. Bore after bore was stopped by the closing in of the sides or by the gravelly nature of the beds, and though no doubt with time and money the deposits could have been pierced, previous experience did not encourage me to persevere, or to hope for any very definite results even if the bottom of the old channel were reached. Attention was therefore mainly devoted to the search for boulder clay beneath the Palæolithic loams, and to the sinking of a trial pit and bore at the point where the buried valley appeared to be deepest.

This exploration tended to show that the story told by the old valley at Hoxne was repeated at Hitchin. Indeed the succession of events at the two localities was so similar as to leave scarcely a doubt that we were dealing with an equivalent set of strata, though unfortunately the series was less perfect, and the gaps discovered at Hoxne are still unbridged by any records yet found at Hitchin. Hitchin yields strong corroborative evidence in favour of the conclusions arrived at in the Hoxne Report, and adds somewhat to our knowledge of the temperate flora of the ancient alluvial strata lying between the chalky boulder clay and the Palæolithic brickearth. I must confess, however, that in other respects the results are somewhat disappointing.

Hitchin lies in the valley cut by the Hiz, a small stream which rises within a mile of the town. Chalk is seen in the valleys, and rises irregularly through the thick sheet of drift that masks most of the area. The greater part of this drift consists of coarse flint gravel, partly of Glacial, partly of Palæolithic age, and one of the greatest difficulties met with is the impossibility of deciding whether any particular pit is in Glacial or in Post-glacial gravel, and whether the gravel passes under or over the chalky boulder clay. The material

being used again and again, the composition of the gravel is practically the same in each case, except for the occurrence of implements in the newer deposits. It was soon found that Mr. Hill was right as to the difficulty of obtaining satisfactory evidence from a study of the gravels. Even between clear sections correlation is impossible, and boring in boulder gravels is so difficult and expensive that I did not feel justified in spending money in attempting to penetrate them to the boulder clay. Even if the boulder clay were reached, it would be fairly open to any critic to say that nothing had been settled, as there is no evidence which of some three or four gravels may be the one represented at that particular spot.

The first thing to be done evidently was to ascertain whether the chalky boulder clay, an undoubted glacial deposit, passes under or over the Palæolithic brickearth. With this object, borings were made in the pits out of which implements have been dug. The details of the borings are given in the Appendix; the results may be shortly summarised thus:—In Ransom's (New) Brickyard, after penetrating the Palæolithic brickearth and underlying fluviatile loam to a depth of 60 feet, some blue, chalky clay was penetrated in BH 2, and in BH 1 derivative fossils from the boulder clay were obtained at about the same level in the lower part of the old alluvium. In each case boring was stopped by gravelly sand full of water. Other borings in Jeeves' Yard and in some old brick-pits on Maydencroft Farm yielded no trace of boulder clay. One only (BH 12) passed through undoubted boulder clay. The section was—

		Feet.
Palæolithic.	Yellow brickearth and small stones.. { dug	9
	{ bored	5½
Ancient Alluvium.	Yellow and white marl and silt.....	2
	Yellow loam and small chalk pebbles	½
Glacial.	Chalky boulder clay	9
	Loamy chalky gravel (base of the boulder clay	2
	Gravelly sand (boring stopped by large stones)	8
		36

The site of this boring is close to the western margin of the old channel, chalk appearing at the surface within a short distance. The occurrence of the chalky boulder clay at this spot at a high level, and its absence, or representation by derivative material, at lower levels in borings made nearer to the centre of the channel, suggest that the channel was, to a large extent, excavated, or re-excavated, after the deposition of the boulder clay, as was the case at Hoxne.

The course of the ancient silted-up channel cannot yet be defined. Loam with Palæolithic implements occupies a belt extending for about a mile south of Hitchin. It has been extensively worked at various times between Windmill Hill and Maydencroft Lane. According to Mr. Hill, implements have also been found on Thistly Farm, half a mile further south. The only pits now worked are those known as "Ransom's" and "Jeeves'."

The underlying river alluvium is entirely overlapped and hidden by the Palæolithic brickearth, and is only to be seen where the brickearth has been dug away. It is found in Ransom's and Jeeves' brickyards, and we have now discovered it, half a mile to the S.S.W., in two borings on Maydencroft Farm. It apparently occupies a narrow belt in the middle of the old valley. The greatest depth of this valley could not be proved, owing to the running sand and gravel, which made boring so difficult. The bottom lies at least as low as the level of the existing valleys of the Hiz and Purwell, for five different borings were abandoned at about that level, without having reached chalk. The trend of the buried channel seems to be from south to north, in this following the general slope of the ground and running parallel to the course of the existing streams.

Work was commenced last November in Ransom's brickyard, one boring being put down at the east corner and another at the south end. The section at the east corner showed 30 feet of Palæolithic loam, resting on 25 feet of carbonaceous loam with freshwater shells, below which came 12 feet of loamy sand and gravel of doubtful age —perhaps glacial, perhaps later. At the south end BH 2 showed—

		Feet.
Dug.	Brickearth with small scattered stones and occasional Palæolithic implements, bedding obscure and irregular	30
	Bedded brickearth with seams of sand (should be searched for Arctic plants; compare Bed C at Hoxne)	8
	White marly sand with freshwater shells and fish teeth	1 to 3½
	Gravelly brickearth	2½ to ½

			Feet.
Bored.	Sandy loamy gravel		9
	Brickearth		2½
	Loamy gravelly sand, full of water		6
	Blue clay and chalk	Base of the boulder clay, or reconstructed boulder clay	3
	Blue coarse loamy sand		1
	Blue hard chalky loam		1
	Sandy gravel and water		3

Other borings in Jeeves' Yard and near Maydencroft Farm yielded

similar results, but only the one already mentioned (BH 12) reached undoubted boulder clay below the alluvial strata.

The relation of the boulder clay to the alluvial deposits having been settled, attention was devoted to an attempt to penetrate to the bottom of the valley in its deepest part. A trial-pit was sunk in Ransom's old brickyard, within a few yards of the spot where a pit had been sunk by Messrs. Prestwich and Evans in 1877, and samples were taken from various depths for washing and minute examination in London. In this brickyard a large number of implements had been obtained from the irregular gravelly base of the brickearth, where it rests on the older alluvial deposits. The newer brickearth, here about 24 feet thick, had been already entirely removed at the spot where the trial-pit was sunk, and it was hoped that the pit could be sunk to the full depth of the old channel. After sinking 14 feet through brown bedded carbonaceous loam, full of badly preserved shells and plant remains, the weather became so unsettled that there was a fear of the pit caving in, or becoming flooded, before the work was completed. Boring tools were therefore used, and a further depth of $17\frac{1}{2}$ feet of alluvial loam was penetrated before the gravelly sand below was reached. At the base of the alluvium was found a foot or so of hard black loam with freshwater shells and fragments of Jurassic fossils derived from the boulder clay. As the quicksand below could not be penetrated without the expenditure of more time and money than could fairly be devoted to such unsatisfactory work, the boring was then abandoned.

In working out the material afterwards, it became evident that from top to bottom the alluvial deposits below the Palæolithic brickearth belonged to one series, the same plants occurring throughout. We have therefore only two deposits to deal with—a stony brickearth yielding little or nothing but Palæolithic implements, and a series of ancient alluvial deposits below full of plants and shells, but, as far as known, without trace of man. The mammalian remains all come, I am informed, from the whitish marly silt which occurs locally immediately below the Palæolithic brickearth. They are mostly in Mr. W. Ransom's collection, and were determined by Mr. Sanford. The remainder of the animals and plants, with the exception of two species of mollusca, were obtained in the trial-pit just described. For the determination of the fishes, I must thank my colleague, Mr. E. T. Newton; for the mosses Mr. W. Mitten; for the mollusca and flowering plants I am responsible.

Mammals.

Ursus; Equus caballus, *Linn.;* Rhinoceros; Hippopotamus (a waterworn bone); Cervus elaphus, *Linn.;* Elephas primigenius, *Blumb.*

Fishes.

Perca fluviatilis, *Linn.;* Esox lucius, *Linn.;* Leuciscus rutilus, *Linn.;* Leuciscus erythrophthalmus, *Linn.;* Tinca vulgaris, *Cuv.*

Insects.

Elytra of several genera of beetles, badly preserved; Galls.

Mollusca.

Helix pulchella, *Müller;* Clausilia biplicata, *Mont.;* Carychium minimum, *Müller;* Planorbis albus, *Müller;* Planorbis carinatus, *Müller* (recorded by Mr. E. T. Newton); Planorbis complanatus, *Linn.;* Planorbis nautileus, *Linn.;* Planorbis nitidus, *Müller;* Planorbis spirorbis, *Müller* (found by Mr. A. S. Kennard); Limnæa auricularia, *Linn.;* Limnæa peregra, *Müller;* Limnæa stagnalis, *Linn.;* Velletia lacustris, *Linn.;* Valvata piscinalis, *Müller;* Valvata cristata, *Müller;* Bythinia leachii, *Shepp.;* Bythinia tentaculata, *Linn.;* Pisidium pusillum, *Gmel.;* Sphærium corneum, *Linn.;* Anodonta.

Flowering Plants.

Ranunculus aquatilis, *Linn.;* Ranunculus sceleratus, *Linn.;* Ranunculus repens, *Linn.;* Montia fontana, *Linn.;* Prunus spinosa, *Linn.;* Poterium officinale, *Hook. f.;* Pyrus torminalis? *Ehrh.* (identical with seeds found at Hoxne); Hippuris vulgaris, *Linn.;* Myriophyllum; Cornus sanguinea, *Linn.;* Sambucus nigra, *Linn.;* Eupatorium cannabinum, *Linn.;* Fraxinus excelsior, *Linn.;* Menyanthes trifoliata, *Linn.;* Lycopus europæus, *Linn.;* Ajuga reptans, *Linn.;* Alnus glutinosa, *Linn.;* Quercus robur, *Linn.;* Ceratophyllum demersum, *Linn.;* Sparganium; Potamogeton crispus, *Linn.;* Potamogeton sp.; Potamogeton sp.; Naias marina, *Linn.;* Scirpus lacustris, *Linn.;* Scirpus sp.; Carex.

Mosses.

Antitrichia curtipendula, *Brid.;* Homalothecium sericeum, *Bry. Europ.;* Hyocomium brevirostre? *Bry. Europ.;* Isothecium?; Neckera complanata, *Bry. Europ.;* Stereodon cupressiformis, *Brid.;* Zygodon?

Characæ.

Several species indet.

Such trees as the oak, ash, sloe, cornel, elder, and alder point unmistakably to a temperate climate, and the fauna and flora as a whole suggest climatic conditions not differing greatly from those

we now enjoy. Mr. Mitten writes of the mosses, that "all these are inhabitants of a sylvan temperate region . . . . and none point to a different environment from that now existing; they are not arctic." The occurrence of *Naias marina*, now only found in Britain in two of the Norfolk Broads, is singular, though the plant was evidently more common in former times than at the present day. It has now been discovered in the pre-glacial deposits of Norfolk and Suffolk, beneath Palæolithic remains at Hitchin, and in a submerged peat of Neolithic date at Barry Docks, in South Wales.

The resemblance of the Hitchin Palæolithic brickearth to the Palæolithic brickearth of Hoxne, and the similarity of the old alluvia beneath, both in fossil contents and in the physical changes they suggest, is so striking that one is compelled to correlate them bed by bed. If, however, this correlation be correct, it is evident that the intermediate deposit full of leaves of Arctic willows, so conspicuous over part of the area at Hoxne, is missing at Hitchin. At each locality the same story is told. Some time after the passing away of the ice the land stood higher than now, so that the streams had a greater fall and valleys were cut to a somewhat greater depth. Then the land sank and the valleys became silted up with layer after layer of alluvium, to a depth of at least 30 feet, the climate remaining temperate. The next stage, when an arctic flora reappeared, is only represented at Hoxne. The third stage in the infilling of the valleys is shown in the curious unstratified decalcified brickearth with scattered stones and Palæolithic implements, identical in character at Hitchin, Hoxne, Fisherton, and other localities, which irresistibly suggests a mingling of wind-transported material and rainwash.

It may be pointed out that if this hypothesis of the origin of the Palæolithic brickearth during the reign of "steppe" conditions be accepted, it will account for the non-correspondence of the ancient channels with the present valleys, a thing very difficult to explain if the infilling were caused by ordinary fluviatile action. If the Palæolithic brickearth is equivalent to the Palæolithic loess of the ancient deserts in central Europe, we can understand how during this period of cold drought the smaller streams ceased to flow and their valleys became so filled with rainwash and dust that when a moister climate recurred the streams had to seek new channels.

APPENDIX.

Borings made at Hitchin in November and December, 1896.

BH 1. In Ransom's new brickyard, east corner. Surface about 268 feet above Ordnance Datum. Work stopped by running sand and water.

		Feet.
Palæolithic.	Brown brickearth, with a few stones and occasional Palæolithic implements; bedding obscure and irregular. (Dug to about 25 feet, bored 5 feet	30
Ancient alluvium.	Brown loam, rather stiffer, with freshwater shells	2
	Brown sandy brickearth and a few stones	11
	Brown sandy brickearth and traces of freshwater shells	4
	Blacker loam. *Bythinia*, *Limnæa*, and boulder clay fossils	7
Glacial ?	Brown brickearth	1
	Gravelly loam	1
	Brown loamy sand and stones	2½
	Blue loamy sand and gravel, full of water	8½
		67

BH 2. In Ransom's new brickyard, south end. Surface about 270 feet (see p. 43).

BH 3. Close to the old brick-pit east of New England. Surface about 235 feet.

		Feet.
	Soil	1
Palæolithic ?	Brickearth and small stones	1
	Gravelly brickearth	1½
	Coarse loamy and gravelly sand	½
	Hard sandy and gravelly brickearth	2
	Loamy gravel	1
		7

No attempt to go deeper was made at this spot.

BH 4. Preliminary trial in old brickpit N.N.E. of Maydencroft Farm (south side of the Charlton Road). Surface about 260 feet.

		Feet.
Palæolithic.	Brickearth (dug 11 feet, bored 2 feet)	13
Ancient alluvium.	White marl with freshwater shells	4
	Blackish loamy sand	½
	Black loam, with freshwater shells and traces of plants	1
		18½

For lower beds see BH 8.

BH 5. Jeeves' Yard, north corner. Surface about 246 feet.

		Feet.
Palæolithic.	Brown brickearth, bedding obscure, occasional Palæolithic implements. (Dug)	11
Ancient alluvium.	Shell marl. A few yards from this spot large bones have been obtained from this bed	9
	Stony brickearth	10
Glacial ?	Fine, rather loamy, gravel, full of water	14
		44

BH 6. Jeeves' Yard, S.W. of BH 5. Level (of present surface) about 240 feet.

		Feet.
	Made ground	1
Ancient alluvium.	Brown sand (decalcified shell marl?)	13½
	Brown brickearth (boring stopped by a large stone)	4
		18½

BH 7. Jeeves' Yard, north-east part, near the pump. Level (of present surface) about 238 feet.

		Feet.
Palæolithic.	Brickearth (already removed)	10
Ancient alluvium.	Shell marl, with *Valvata piscinalis*, &c.	8
	Black loam with fish-bones, freshwater shells, seed of alder, and *Naias marina*	7
	Thin seam of small lignite.	
	Laminated brown and white loam, more sandy. Obscure vegetable remains	10
	Lighter coloured very soft loam	2
		37

BH 8. In old brickpit N.N.E. of Maydencroft Farm and on south side of the Carlton Road (close to BH 4). Level of old surface about 260 feet.

		Feet.
Palæolithic.	Brickearth, already removed	14
	do. dug	1
Ancient alluvium.	White calcareous silt, with carbonaceous seams, *Bythinia*, and fish-bones	2
	Yellow loam, darker below	3
	Seam of fish-bones.	
	Yellow loam	½
	White silt / Brown silt	½
	Black loam with freshwater shells and seeds	7
	Stiffer black loam with traces of plants	4
	Stiff, hard, blue-black clay with small roots	½
Glacial?	Coarse, blackish, loamy sand	½
		33

BH 9. Road-side, close to paths to Maydencroft Farm and Priory Park. Surface about 273 feet.

		Feet.
	Brickearth and numerous stones	3

BH 10. Close to BH 12 (which see).

BH 11. Roadside south of Priory Park, ¼ mile east of Charlton.

		Feet.
	Soil	1
Ancient alluvium, decalcified? or Glacial.	Sandy and stony brickearth	3
	Yellow sandy gravel	3
	Yellow sand	4
	Yellow gravelly sand	1
	Loamy sand	4
	Laminated reddish-brown loam	2
	Sand, darker and rather loamy	3
		21

BH 12. In old brick-pit 90 yards S.S.W. of the Charlton road and close to path to Maydencroft Farm. Level (of old surface) 271 feet.

		Feet.
Palæolithic.	Yellow brickearth with small stones (9 feet already removed)	14½
Ancient alluvium.	Yellow and white marl and silt (compare BH 8)	2
	Yellow brickearth and small chalk pebbles	½
Glacial.	Chalky boulder clay	9
	Loamy chalky gravel	2
	Gravelly sand	8
		36

BH 13. Old gravel pit in Jeeves' Yard, near Black Horse Lane. Level (of old surface) 242 feet.

		Feet.
Glacial.	Coarse gravel, becoming finer below. (Dug)	18
	Finer gravel, watery below. (Bored)	10½
		28½

BH 14 and trial-pit (see p. 44).

"Luminosity and Photometry." By JOHN BERRY HAYCRAFT, M.D., University College, Cardiff. Communicated by Professor SCHÄFER, F.R.S. Received February 9,—Read March 4, 1897.

The luminosity of the spectrum was determined by what may be termed the method of the "minimal effective stimulus." Through the partition separating two dark rooms from each other a hole was drilled 1 mm. across; this was covered with a piece of ground glass. From a diffraction grating in room A spectral rays of ascertainable wave-length were cast upon the ground glass. The observer in room B was placed upon a graduated railway, and moved towards or from the spot of light until he could just see it, and no more. The distances at which various parts of the spectrum were just seen were thus determined, and their relative potencies calculated by the law of inverse squares. Under the above conditions—the eye being in the condition of dark adaptation—the green near E is the most potent, and the violet half of the spectrum is more potent than the red. These results agree with those obtained by Captain Abney who also worked with the dark adapted eye.

The experiment was then repeated, the room B being white-washed, and lit with gas light, and the eye of the observer, therefore, in the condition of light adaptation. Under these conditions the yellow is the most potent, and the red half of the spectrum more potent than the violet.

In the following experiment the eye was in the condition of

light adaptation for strong stimuli, and passed gradually into the condition of dark adaptation as the stimuli became feebler. Tiny discs of coloured paper were pasted on a black background, and the distance determined at which these spots could just be seen. The room in which both the discs and the observer were placed was illuminated by a graduated gas burner reading from twenty candles downwards. For low luminosities when the quality of the flame changes, the burner was raised towards the ceiling to measurable distances.

Order of potency of discs; distance at which they were seen.

At high illuminations	Yellow, red, green, blue.
„ low „	Yellow, green, red, blue.
„ very low illuminations ..	Green, yellow, blue, red.

The potency of different portions of the spectrum was also determined by the method of flicker photometry. A semi-disc, rotated by an electromotor between the source of light and the slit of the spectroscope, repeatedly cut off the light entering the instrument. The rate of rotation of the semi-disc was varied by the use of a graduated resistance, and the rate could be accurately determined. At a low speed of rotation the spectrum flickered, except at its ends; the position of these two ends was determined. On increasing the speed of rotation, the flickering centre was narrowed, and another two points were obtained. From a number of such observations a curve, giving the potency of the spectrum to produce flickering, was constructed. With a dim spectrum the green, near E, is the most potent, and the violet half of the spectrum is more potent than the red. On increasing the luminosity of the spectrum, the apex of the curve shifts from the green into the yellow, and the red half becomes more potent than the violet. Flicker curves from coloured papers observed at different luminosities were also obtained. The graduated gas-burner was here utilised, and throughout these experiments, gas-light was used as the source of light. The curves obtained by flicker photometry, and those obtained by the method of the minimal effective stimulus, strikingly resemble each other. The nature of the flicker effect is discussed, and the curves obtained by the above methods are given in a paper shortly to appear in the 'Journal of Physiology.'

March 11, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Comparative Physiology of the Suprarenal Capsules." By SWALE VINCENT, M.B. (Lond.), British Medical Association Research Scholar. Communicated by E. A. SCHÄFER, F.R.S.

II. "The Origin and Destination of certain Afferent and Efferent Tracts in the Medulla Oblongata." By J. S. RISIEN RUSSELL, M.D., M.R.C.P., Research Scholar to the British Medical Association, Senior Assistant Physician to the Metropolitan Hospital, and Pathologist to the National Hospital for the Paralysed and Epileptic, Queen Square, London. Communicated by Professor VICTOR HORSLEY, F.R.S.

III. "On the Orientation of Greek Temples and the Dates of their Foundation derived from Astronomical Considerations, being a Supplement to a Paper published in the 'Transactions of the Royal Society,' in 1893." By F. C. PENROSE, F.R.S.

IV. "Some Experiments with Cathode Rays." By A. A. C. SWINTON. Communicated by LORD KELVIN, F.R.S.

V. "A Study of the Phenomena and Causation of Heat-contraction of Skeletal Muscle." By T. G. BRODIE, M.D., and S. W. F. RICHARDSON, M.B., B.S. Communicated by W. D. HALLIBURTON, F.R.S.

"The artificial Insemination of Mammals and subsequent possible Fertilisation or Impregnation of their Ova." By WALTER HEAPE,* M.A., Trinity College, Cambridge. Communicated by FRANCIS GALTON, F.R.S. Received January 15,—Read February 11, 1897.

My attention was first drawn to this subject in consequence of the results obtained during an investigation, with which I have been occupied during the last three years, upon ovulation in the rabbit.

I found that "heat" alone was not sufficient to cause ovulation in the virgin rabbit, but that $9\frac{1}{2}$ to 11 hours after copulation ovulation almost invariably took place. I then endeavoured to determine whether the act of copulation or the presence of spermatozoa in the female generative organs were independently sufficient to induce ovulation in does which were on "heat," and I have made a considerable number of experiments for that purpose, an account of which I hope shortly to publish. Amongst them I made efforts to inseminate artificially virgin does when on "heat." Seminal fluid was injected into the vagina of several of these animals, and in certain of them spermatozoa were subsequently found in the uterus and fallopian tubes. In no case, however, did the presence of spermatozoa thus artificially introduced induce ovulation, and it would seem that the presence of spermatozoa alone is not sufficient for the purpose.

Again, in a case where copulation took place, and where the os uteri of the doe was obstructed so as to stop the passage of spermatozoa into the uterus, ovulation did not take place, and thus it would seem that the act of copulation is not of itself sufficient for the purpose.

Such experiments as I have already made do not entirely satisfy me, but so far as they go they point to the conclusion that both copulation and the presence of spermatozoa in the uterus are necessary to induce ovulation in the virgin rabbit when she is on "heat."

Ott (No. 24) experimented on sixteen rabbits, and claims to have induced gestation in one of them by artificially introducing spermatozoa through the body-wall into the peritoneal cavity near the ovary. His description is not very clear, but it would appear probable that he used for the experiment a doe rabbit which had kindled shortly before, and it is asserted by Weil (No. 39) that ovulation takes place spontaneously at such times in rabbits. This is a very interesting point, but I will not allude to it further here, as it properly concerns questions of ovulation and the maturation of ova which I propose to treat at length elsewhere.

* With this is incorporated an account of experiments performed by Sir Everett Millais, Bart., and communicated by him to the author.

Certain other mammals, however, differ from the rabbit in the matter of ovulation. Sobotta (No. 34) has shown that mice ovulate prior to the act of copulation, and Tafani's (No. 36) researches point to the same conclusion for rats, though whether the animals used for these investigations were virgins or not is not stated.

Spallanzani (No. 35), Pierre Rossi (No. 27), and Sir Everett Millais's experiment on bitches, to which I refer below, show that the act of copulation is not necessary to bring about ovulation in these animals; certain other writers have shown that the same is true for the mare, and the researches of various medical experts demonstrate a similar possibility for the human female.

According to Hausmann (No. 12), sheep do not ovulate unless copulation takes place, but it is not improbable that many, if not most, of the lower mammals will be found to ovulate independently of the act of coition. In order to prove this fact, as my researches on the rabbit will show, other evidence must be sought for than the fallacious evidence of the appearance of the so-called corpora lutea in the ovaries (Pouchet, No. 26).

The possibility of artificially inseminating mammals in which ovulation takes place independently of coition, has seemed to me well worthy of notice, and the following short account has been written for the purpose of drawing attention to a subject which has apparently been much neglected, and with the hope that it may induce observers to record anew experiments of which I have been unable to find records and facts bearing upon it which are not already known. The importance of the subject is not, I think, fairly recognised, but I believe that a fuller knowledge of what is known about it will show that it is a matter of great interest both from a scientific and a practical point of view.

It has been ascertained that if spermatozoa be placed artificially in the vagina of certain female mammals at the right season, they may conceive; it has been ascertained also that if spermatozoa be placed artificially within the uterus of certain individual mammals which have failed for particular reasons to breed by natural means, they may become pregnant in consequence; and there is little doubt that an extended study of the subject will throw light on the physiological relations of coition and insemination, ovulation and fertilisation, and on certain of the causes which induce sterility in mammals, which will be of great interest to physiologists and of great value to practical breeders.

There is another, a wider but more problematical direction in which a fuller knowledge of the possibilities of artificial insemination may induce experiment; I mean the crossing of varieties or of species that will not naturally breed together.

I am not aware of any proof that any mammalian spermatozoon is

precluded from fertilising any mammalian ovum. The indiscriminate crossing of mammals is sufficiently guarded against naturally by the variation in size, in habit, and in the formation of the generative organs of the different species; it remains to be proved that the spermatozoa of one species will not fertilise the ova of other species and that the maternal organ will not receive and nourish the hybrid embryo.

I would here draw attention to the use of the word "inseminate." The terms "fertilisation" or "impregnation" have very generally, though as I believe erroneously, been used to denote the fact that the male generative substance has been introduced onto or into the generative organs of a female plant or animal, and it has even been assumed by some authors that the female herself has thus become "fertilised," or "impregnated." This assumption has probably given rise to some confusion, those who believe in telegony may, likely enough, be influenced by it. As far as is known, the female absorbs no essential part of the male elements with which she is supplied, although it may be found that she does so. However that may be, the female herself is not fertilised by means of pollen or spermatozoa, and her ova are not necessarily impregnated in consequence of the introduction into her generative organs of the male element. It has seemed necessary, therefore, to make a distinction between the introduction of seminal fluid into the female generative organs of animals and the subsequent possible fertilisation of their ova, and for that purpose I have used the word "inseminate," which can thus be applied to animals in precisely the same way as the word "pollenate" is applied by some botanists to denote the placing of pollen on the stigma of a plant.

Some time about the year 1770, Spallanzani (No. 35) made experiments on the artificial insemination of various oviparous animals and subsequently was induced to attempt a similar experiment on a mammal. He chose a bitch of the "Barbet" breed for the purpose and kept her in a room for a long time, feeding her himself.

She came "on heat" in due time and after the bleeding stage was over, showed strong desire for coition. Spallanzani then obtained from a young dog of the same breed, 19 grains of seminal fluid, and this, he says, he injected without delay into the "matrice" of the bitch by means of a small pointed syringe, taking care to keep the instrument the same temperature as the seminal fluid, viz., about 30° Réaumur. Two days after the injection the period of "heat" was finished; twenty days later the abdomen seemed swollen, and on the twenty-sixth day the bitch was set at liberty. The abdominal swelling increased, and sixty-two days after the injection was administered she gave birth to three pups.

On the 12th January, 1782 [according to Tarnier (No. 37), sixteen

months after Spallanzani had completed the experiment mentioned above], Pierre Rossi (No. 27) repeated this experiment. He also chose a "Barbet" bitch for the purpose. When she appeared to be coming "on heat" he locked her up in a room, to open which two keys were necessary; one of these he handed to M. Nicholas Branchi, Professor of Chemistry in the University of Pisa, and the other he held himself.

On the 25th January the bitch was ready for coition and spermatozoa was injected into her in the same way as Spallanzani describes his experiment. The injection was repeated on the 26th, 28th, and 30th of the same month. On the 1st February "heat" ceased, on the 26th the abdomen was swollen and the mammæ enlarged, and the bitch was set at liberty. On the 27th March, sixty-two days after the first injection, she littered four young.

The care with which both, but especially the latter, of these experiments was conducted, leaves little room for doubt that the bitches produced young in consequence of artificial insemination.

It is a matter for surprise that such extremely interesting experiments did not stimulate research in the same direction. They do not, however, appear to have done so, at any rate I can find no reference since Spallanzani's day until quite recently, of any experiments on the artificial insemination of the lower mammals.

The human female, on the other hand, has been made the subject of like experiments by various medical men. Hunter (No. 14), in 1799, seems to have been the first to advise the artificial insemination of woman in order to insure pregnancy. In the case which he records, the operation was performed in consequence of the fact that the husband of the woman suffered from hypospadias, and the seminal fluid was merely injected into her vagina. It is recorded that success attended the operation and that the woman became pregnant and bore a child.

Since Hunter's day various medical men have practised artificial insemination on women who were supposed to be sterile, with more or less success. Their method has been, to inject spermatozoa into the uterus of the woman by means of a specially devised instrument and in that way to overcome certain defects or obstructions in the female generative organs; such as flexion or constriction of the canal of the cervix; rigidity of the cervix; hypertrophy of the lips of the external os which may effectually close the orifice; inability to retain the spermatozoa in the vagina owing to violent muscular contraction, or to retroversion of the uterus (Sims, No. 33); a want of sufficient muscular power; abnormal structure or size of the cervix or os uteri which prevents the free action of the functions of the cervix ["aspirations," Courty (No. 3)]; abnormal or excessive vaginal secretions, &c.

Such defects as these may effectually prevent conception by natural means, since spermatozoa placed in the vagina may thus be prevented from ascending to the uterus and fallopian tubes and from meeting there a newly discharged ovum, but they may be overcome by placing the spermatozoa directly into the uterine cavity.

The great difficulty of ensuring success with women would seem to be due to our ignorance of the time of ovulation. I will not refer to this matter further here, having considered it somewhat fully elsewhere (No. 13).

The performance of the operation on women presents no great difficulties and writers on the subject are agreed on this point. Spermatozoa is collected, either direct from the male, or from the vagina of the female immediately after coition, and is injected into the cavity of the uterus by means of a syringe, to which is attached a fine tube which the operator passes through the canal of the cervix.

Tarnier (No. 37) credits Girault with having operated on forty women who were positively declared to be sterile, of which number eighteen bore children in consequence, and his seems to have been the most successful record. I have given at the end of this paper a list of the authors who deal with the subject, and it will not be necessary to go more fully into this branch of it here, since the writers record nothing which is of further general interest.

In recent years both bitches and mares have been successfully inseminated by artificial means.

Sir Everett Millais, who is a noted breeder of Basset hounds, has made a very remarkable series of experiments of this kind on bitches in his own kennels, and he has most kindly supplied me with an account of them and placed the details at my disposal for publication. His method is to collect spermatozoa from the dog and inject it by means of a syringe into the generative canal of the bitch; it seems probable that the syringe does not reach beyond the vagina and that the spermatozoa is thus deposited at the further end of that organ, from whence it finds its way into the uterus by the same means employed after natural insemination. These experiments were not undertaken in order to overcome barrenness; hence it was quite sufficient for his purpose to inject the spermatozoa into the vagina.

Experiment 1.—In 1884 Sir Everett Millais published (No. 19) an account of the first experiment he made. He inseminated artificially a Basset hound bitch with spermatozoa obtained from a dog of the same breed, with the result that the bitch gave birth to pups, which were, however, born dead.

Experiment 2.—In 1885 he artificially inseminated three Basset hound bitches, the semen being obtained from a single emission of a dog of the same breed, and each bitch receiving one-third part of it.

All these bitches pupped successfully, one having a single puppy, another five puppies, and the third seven puppies.

Experiment 3 (1887) was a failure; the bitch used for this experiment invariably refused to allow a dog to serve her when "on heat," and although she offered no objections to the performance of artificial insemination she failed to conceive.

Experiment 4 (1891). A Bloodhound bitch was artificially inseminated with material obtained from the Basset hound dog "Floreal;" a single puppy was the result, and it resembled anatomically its sire.

Experiment 5 (1891) was not satisfactory, a miscarriage taking place.

Experiment 6 (1891) was a failure; the bitch was a noted bad breeder and failed to conceive.

Experiment 7 (1891) was a failure; the dog used was a very old one and it is possible the failure was due to him.

Experiment 8 (1891) was also a failure, and in this case also the bitch used was a bad breeder, having only produced one puppy in her life.

Experiment 9 (1892). The Basset hound bitch "Antoinette" was artificially inseminated with semen from the dog "Floreal," and a litter of seven puppies was the result, one of them, "Syringa," being as he says, "the best puppy I ever bred."

Experiment 10 (1893). A Bloodhound bitch was artificially inseminated with semen taken from a Basset hound and immediately afterwards she was served naturally with a dog of her own breed, "Dandy." This bitch littered eleven puppies, eight of which were pure bred Bloodhounds, and three were half-bred Basset and Bloodhounds.

Experiment 11 (1893). The Basset hound bitch "Bud" was artificially inseminated with semen from the dog "Floreal," six puppies resulting.

Experiment 12 (1893). A Bloodhound bitch was inseminated artificially with semen from the Basset hound "Nicholas." She had twelve puppies, all resembling their sire anatomically. As this bitch was dying in labour, these puppies were delivered by Cæsarian section, and seven of them were reared.

Experiment 13 (1894). A Bloodhound bitch, "Garnet," was artificially inseminated with semen from the Basset hound "Floreal." One bitch puppy, "Bella," was born; she resembled anatomically her sire, and she has since been bred from.

Experiment 14 (1894). A Basset hound bitch, "Juno IV," was artificially inseminated with semen from a Basset hound dog champion, "Forrester," and eight puppies were born in consequence.

Experiment 15 (1894). One of the half-bred bitches obtained by Experiment 12 was artificially inseminated with semen from the dog "Floreal," and she littered nine puppies.

Experiment 16.—A three-quarter bred bitch, "Betsy," whose dam was one of the puppies resulting from Experiment 12, was artificially inseminated with semen obtained from the Basset hound dog champion "Forrester," and nine puppies were born.

Experiment 17 (1896). The artificial insemination of another Basset hound bitch has resulted in the birth of puppies.

Nineteen bitches were artificially inseminated in these seventeen experiments, and only four of them failed to conceive. Of these four, one bitch invariably refused the dog, two bitches were known to be uncertain breeders, and the fourth was injected with semen obtained from an old dog and it is possible the failure to breed in this case may be due to the dog and not to the bitch.

This remarkable series of experiments, extending over twelve years, is of very great interest. All possible care was taken by the owner, an experienced breeder, whose kennels are admirably organised, that no error should creep in, and it may be confidently stated as a result of them—

First, that artificial insemination of bitches can readily be performed; secondly, that pregnancy can be as readily induced by that means as by the natural method of coition; and, thirdly, that a single emission of spermatozoa by a dog can be made use of to inseminate artificially several bitches, all of which may be expected to bear pups.

Another interesting fact which is demonstrated by these experiments is the facility artificial insemination affords of obtaining crosses between animals which, on account of the great difference in their size (as in Basset and Bloodhounds) cannot readily be obtained by natural means; while a further matter of interest, of no little importance to students of heredity and telegony, is the demonstration of the possibility of inseminating a single bitch with the semen of two or more dogs of different breeds at the same time, and of inducing her by this means to produce a variety of crosses in a single litter.

Gautier (No. 7) also claims to have artificially inseminated a bitch, but he gives no details of the operation, and I have been unable to find records of any other experiments of this nature on dogs.

With regard to horses, Gautier (No. 7) cites Dr. Le Bon as responsible for a quotation from a book written in the year 700 of the Hejira, from which it appears possible that artificial insemination of mares was not unknown to the Arabs in those days.

He describes how the owner of a valuable mare "on heat," armed with a handful of cotton-wool which had been saturated with the discharge from the vagina of the mare, approached by stealth a valuable stallion belonging to a member of a neighbouring hostile

tribe, and, having sufficiently excited the animal with the scent of the material he had brought, collected from him spermatozoa, which he introduced on his return into the vagina of his mare, and obtained thereby a foal.

The story as it stands is sufficiently improbable, but it would seem almost as improbable that such an incident as is here described could have been invented or imagined in those early days, and, in view of the fact that artificial insemination of mares is actually practised now, though in a very different manner, the improbability of this story is, to some extent, decreased.*

The only modern evidence I have been able to obtain regarding the artificial insemination of mares, comes from the United States.

A writer on "Breeding Mares," in 'The Horseman' (No. 1), 1894, speaks of the artificial insemination of mares as of established usefulness, giving positive results.

He gives as an example his experience on a farm where the stallion was of "faulty formation," and the mares were not "settling." At the time of his visit four mares were in season; by his advice one of them was served by the stallion, and the other three were artificially inseminated with semen taken from the vagina of this first mare.

The result of the experiment was that the mare which was actually served by the stallion failed to become pregnant, while the three which were artificially inseminated were all got with foal. Subsequently, the writer adds, twenty-five mares on this same farm, which had previously missed getting with foal in the ordinary way, were operated on, and twenty-three of them became pregnant in consequence.

A writer on "Artificial Impregnation," in the same paper (No. 2), 1895, quotes a letter from a breeder in Harrison, Ill., dated August, 1894, from which it appears that the previous season this gentleman had seventeen mares which had refused to "stand to the horse." All of them had refused for two years past, some for several years; these mares were artificially inseminated and nine foals were in consequence produced.

In this article it is recorded that one mare, sixteen years old, which had repeatedly failed to produce young in the ordinary way, was operated on in July, 1893, and bore a filly in June, 1894; while a jennet, twenty-five years old, which had not bred for six years, foaled a colt in June, 1894, after artificial insemination in June of the previous year.

There can be little doubt of the *bona fides* of these reports, but in

* *Note, February* 11.—In connection with this story I should have added here, that the spermatozoa of a dog was sent to me by post, during the early spring of 1896, and, when examined eighteen hours after it was obtained from the dog, I found fully half of the spermatozoa active and vigorous.

order to confirm them, Professor Pearson, Veterinary Professor at the University of Pennsylvania, Philadelphia, was applied to, and Mr. Francis Galton very kindly forwarded to me a letter from Professor Pearson, in which he states that the artificial insemination of mares is practised on a number of stock-farms in America with fairly satisfactory results; and further, he adds, he himself has performed the operation and attained success.

The usual method adopted is, to allow the stallion to cover a mare in the ordinary way, then, immediately after coition, to pass a hand into the vagina carrying therein the nozzle of a specially constructed syringe, to depress with the tips of the fingers the ventral wall of the vagina at its anterior end, and draw up into the syringe the seminal fluid which will collect in the depression so formed. The same mare can then be inseminated by inserting the nozzle of the syringe through the external os uteri and forcing some of the semen into the uterine cavity, then the syringe can be withdrawn, and other mares which are "in use" can be inseminated with the remainder of the semen in a similar way.

Another method is also in use in the United States, and in this case gelatine capsules are used instead of a syringe; these capsules are filled with spermatozoa from the vagina of a mare which has been covered; they are then closed and gently forced through the external os into the uterus and allowed to remain there; the heat of the body melts the gelatine, and the semen is set free.

It is possible by either of these methods to inseminate several mares with semen obtained from one single act of coition, and in this way the usefulness of a valuable stallion may be vastly increased.

The above operations on mares were conducted for the purpose of overcoming certain causes which induced sterility, and for this reason the semen was introduced directly into the uterine cavity. What these causes actually were is not stated, but it is obvious that certain defects which prevent mares from breeding in the ordinary way can be overcome by artificially introducing the spermatozoa of a horse into their uterus.

The causes of sterility in mares are, apparently, very various. Fleming (No. 6), in the couple of pages which he devotes to the subject, briefly refers to many of them. Here we are only concerned with the causes which prevent the passage of live spermatozoa from the vagina into the uterus of an otherwise normally constituted animal.

An abnormal condition of the cervix uteri is more common than is generally known, according to Fleming; occlusion of the os, due to a spasmodic contraction of the muscles of the cervix during coition; rigidity of the cervix; hypertrophy of the tissue of the os; and the formation of false membranes, which serve to close the entrance to

the uterus; are all causes which may be overcome by the method of artificial insemination described above.

Then, again, abnormal vaginal secretions may kill the spermatozoa; and another not uncommon cause of sterility in the mare, not mentioned by Fleming, is the inability of the vagina to retain the semen after coition; in such cases the withdrawal of the horse is immediately followed by ejection of the semen from the vagina. This fault may be due either to violent muscular contraction of the vagina, or to an abnormally short vagina which is stretched by the penis during coition, and which springs back again rapidly when that organ is withdrawn. Artificial insemination is readily applicable also in the latter case; the semen is collected in a basin as it falls from the vagina, is rapidly drawn up into a syringe, and injected into the uterus in the way described above.

With regard to the number of cases of sterility in mares, there is no doubt that the percentage varies in different years, and that some seasons appear to be more favourable for breeding than other seasons. According to the evidence obtained by the Royal Commission on Horse Breeding (Nos. 29, 30, 31, 32) it is probably correct to estimate that about 40 per cent. of breeding mares which are put to the horse fail to produce offspring each year.

This is a very considerable proportion, and it would appear highly probable that breeders of valuable animals would gladly adopt a method of reducing the loss occasioned by the infertility of so large a proportion of mares, provided they could be assured that harm cannot be done to breeding stock by the process, and provided also that the offspring so obtained should be as healthy and vigorous as that got in the ordinary way.

The operation itself is perfectly simple, and cannot possibly do harm if conducted by a qualified person; it causes neither pain nor inconvenience to the mare.

With regard to the offspring begot by this process, I have no evidence bearing on their vigour or health, and no doubt it is important to the breeder to know this. The best way of solving the question would be to use thoroughbred mares for the experiment, and to subject the foals so obtained to the same rigorous tests usually accorded to such animals; their health, vigour, and stamina would by this means be readily and justly judged.

By the kindness of Lord Rosebery I was enabled late last spring to superintend the artificial insemination of the thorough-bred mare "Myra." This mare has proved an uncertain breeder, and during the whole of last breeding season she had failed to stand to the horse. She was covered in my presence, and the seminal fluid drawn up from her vagina into a syringe; some slight difficulty was at first experienced in passing the nozzle of the syringe through the os,

owing probably to hypertrophy of the lips of the os, but the difficulty was readily overcome without annoyance to the mare and she was inseminated. Unfortunately the experiment failed and she has not proved to be with foal.

Whether the failure in this case was due to abnormal vaginal secretion, with which, of course, the semen was mixed when injected into the uterus, or to the absence of ova, or to faulty powers of gestation, I have no means of knowing. Many experiments are needful in order to enable one to form a satisfactory opinion on such points; but there is good reason to believe that, with experience, they may be determined, and it is greatly to be hoped that breeders will be found who are willing to allow such of their stock as are unsatisfactory breeding animals, to be treated in this way before finally discarding them as useless.

Cows are frequently sterile for reasons similar to these detailed for the mare, hypertrophy of the mucous membrane of the os uteri and consequent closure of the cervix canal being a common cause of sterility in these animals.

It is said that artificial insemination can be performed on cows as readily as it can on mares, but I have seen no published account of experiments on cows.

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"The Comparative Physiology of the Suprarenal Capsules."

By SWALE VINCENT, M.B. (Lond.), British Medical Association Research Scholar. Communicated by E. A. SCHÄFER, F.R.S. Received February 18,—Read March 11, 1897.

(From the Physiological Laboratory, University College, London.)

Notwithstanding the belief of Cuvier, "qu'il était probablement réservé à l'anatomie comparée d'expliquer le véritable usage des capsules surrénales," this method of comparative investigation has been but little employed in the attempt to discover the functions of these organs.

It has, in fact, been doubtful as to what are to be regarded as suprarenals and what are not, in certain of the lower vertebrates, especially in fishes. Thus it has been suggested that the lymphatic "head-kidney" of Teleosts may represent these bodies (Weldon, 19, 20); even the presence or absence of suprarenals in any given order has often been a matter of considerable doubt. I have, in previous papers (15, 16, 17, 18) endeavoured to clear up some of these points, and have described the anatomy and histology of the suprarenal capsules in fishes, amphibians, and reptiles, and I hope in the present communication to give some experimental confirmation of the correctness of the opinions I had previously maintained from histological considerations.

It will be advisable to prefix a few words about the comparative anatomy of the suprarenal capsules. Suprarenals of some sort are probably present in most, if not all, vertebrate animals. In the Cyclostomata this is at present doubtful,* so that it is not until we come to the Elasmobranch fishes that we find with certainty anything in the way of suprarenals. Here we meet with two kinds of structure concerning which there has been much discussion. In the first place, we find a series of paired bodies arranged segmentally on the intercostal arteries, and extending the whole length of the abdominal cavity. They are situated in close proximity to the sympathetic nervous system. Secondly, we have a single or paired, yellow, rod-shaped organ lying between the two halves of the kidney and near the dorsal aspect of this organ. This is the "interrenal" of Balfour (1).

The histology of these two organs I have described elsewhere (*loc. cit.*), and also I have expressed my belief that the paired segmental bodies correspond to the medulla of the suprarenals of higher vertebrates, while the interrenal body corresponds to the cortex. That this was the case was surmised long ago by Leydig (6, 7), and it is

* See, however, Collinge and Vincent, 'Anat. Anz.,' vol. 12, Nos. 9 and 10, 1896.

experimental evidence in favour of this view which I now wish to put forward. Before doing so, however, it will be well to state here that, so far as I have been able to make out, one only (viz., that which corresponds to the cortex) of the two suprarenal constituents is present in Teleostean fishes. The same probably applies to Ganoids.

With regard to the development of the suprarenals it is only necessary to note that many observers believe that the medullary portion is derived from, or at any rate, developed in connection with, the sympathetic nervous system (Balfour, 1), and it seems clear from the researches of Mihálkovics (8) that the cortex is developed from the germinal epithelium.

The researches of Oliver and Schäfer (9, 10, 11), followed by those of Cybulski and Szymonowicz (2, 3, 4, 13, 14), have shown that the medulla of the suprarenal capsules of mammals and the suprarenal capsules of birds and amphibians (Szymonowicz, 14) (presumably the medulla only also in these), produce a remarkable and characteristic rise of blood-pressure, when an extract is injected into the circulation of a living animal. But, so far as I know, no one has previously tested the effects of extracts of the suprarenal bodies of fishes.

The following experiments were accordingly undertaken at the suggestion of Professor Schäfer, to whom I am indebted for advice and assistance on many points connected with the research. I have already published a preliminary notice (17), giving the results of initial perfusion experiments. I have now repeated and confirmed these perfusion experiments, and, in addition, have tested the effect of the materials in question upon the arterial system of mammals.

Effects of Extract of Fishes' Suprarenals upon the Arterial System.—The methods employed have been :—

1. The perfusion of normal saline solution or Ringer's circulating fluid containing the extract to be tested, through the blood vessels of large toads after the brain and spinal cord had been destroyed by pithing.

2. The injection of the extracts into the blood-vessels of a living mammal and recording in the usual way the blood-pressure tracing with the mercurial kymograph. Dogs and cats have been used in these experiments.

The suprarenals employed in this research have been obtained mainly from *Scyllium canicula* among the Elasmobranchs and from *Anguilla anguilla* as a representative of the Teleosts, but I have also used *Scyllium catulus*, *Acanthias vulgaris*, *Galeus canis*, and others in the first-named order, and *Gadus morrhua* and several other species of Teleosts. The effects produced upon blood-pressure were practically identical in the corresponding organs of different species.

The extracts employed were obtained in various ways. Some were prepared by pounding the glands with sand and normal saline in a pestle and mortar, and subsequently filtering. Others were alcoholic, while still others were got by boiling for a short time a certain quantity of the material in a known amount of normal saline and filtering. In all cases care has been taken to obtain the solution free from particles before injection or perfusion.

For intravenous injection an extract of 1 in 25 of the fresh, moist gland has been usually employed. I have not ascertained the minimal effective dose, but 1 c.c. of such an extract (= 0·04 gram of the fresh gland) from the active glands produced a powerful effect.

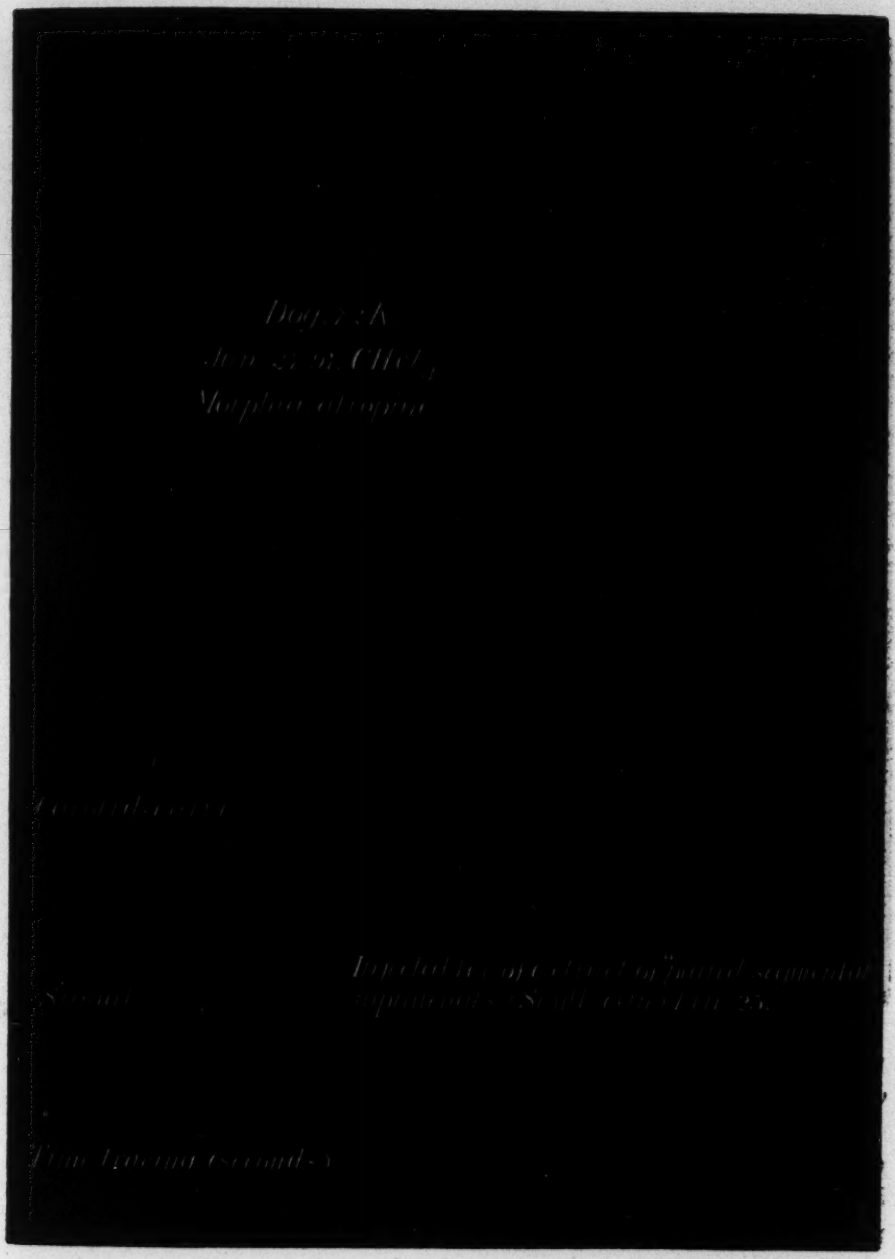
The results obtained by these two methods are quite harmonious. I had anticipated the possibility that the extract from a fish might be inactive upon a mammal, but it will be seen that this anticipation was unfounded. There is in all cases, where "medullary" substance has been injected, very striking evidence of contraction of the arterioles throughout the body. This was made manifest when using dogs or cats with the mercurial kymograph by an enormous rise of blood pressure (see fig. 1). In the perfusion experiments upon toads the result was seen in an almost complete cessation of flow of fluid through the blood-vessels (see Experiment 1). When "cortical" substance was employed little or no effect in these directions was obtained (figs. 2 and 3, and Experiments 1 and 2). It is true there was always a slight rise of blood-pressure or a small diminution of flow of fluid when extract of interrenal was used (Experiment 1 and fig. 2), but an explanation of this is, I believe, readily to be found. The extracts from the suprarenals of Teleosts have always given negative results when tested by both methods (see Experiment 2 and fig. 3).

The effect of the active principle upon the arterioles is due to a direct action upon the muscular tissue of the blood-vessels, and is not in any way connected with the action of the central nervous system. This is perfectly clear from the fact that the effects are well marked in the toad when brain and spinal cord have been destroyed by pithing. Oliver and Schäfer found this to be the case with mammalian suprarenal extract, though Szymonowicz and Cybulski maintain the contrary. The results of Oliver and Schäfer have since been completely confirmed by Velich.*

Fig. 1 shows the most typical effect; the lever rises gradually at first, then afterwards almost vertically. In some cases the pressure was too high to be recorded, as the mercury escaped from the end of the manometer. The effect passes off after a variable period, and the blood-pressure returns to normal. There is no need to give further details, but it will be sufficient to say that the experiments

* 'Wien. Med. Blätter,' Nr. 15, bis 21, 1896.

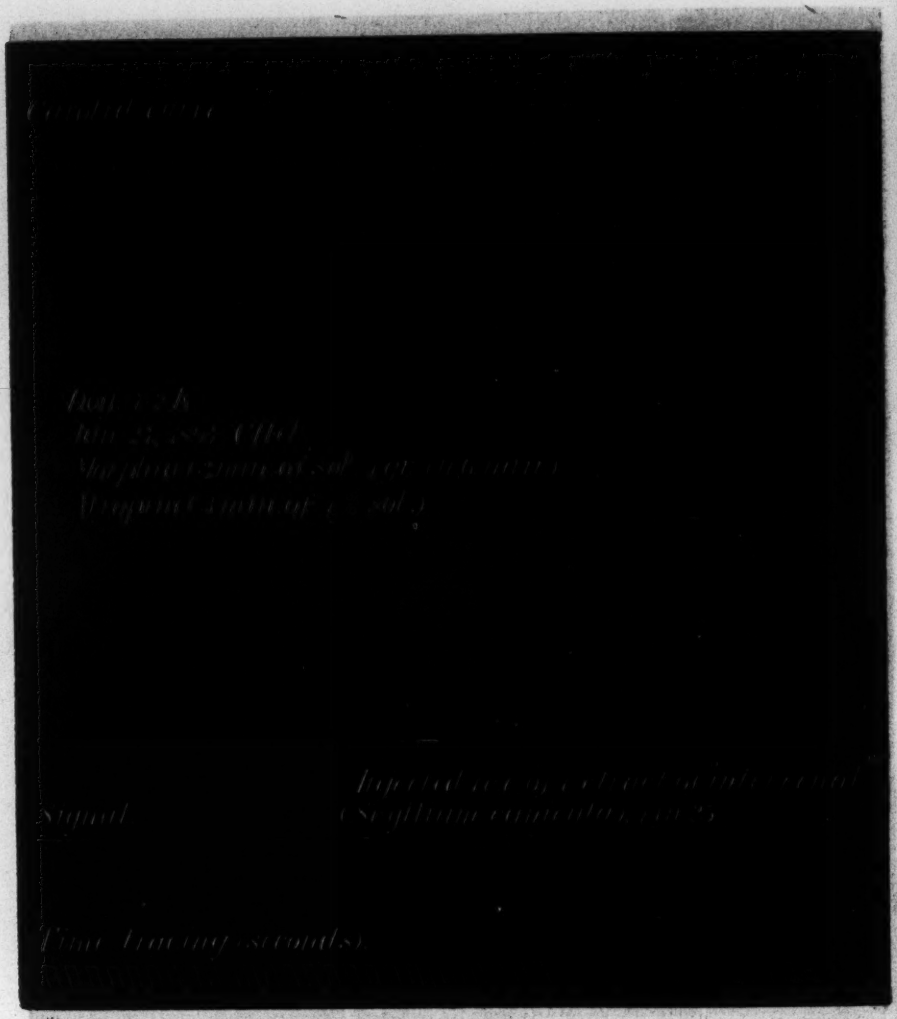
FIG. 1.



all point to the fact that the "paired segmental" suprarenal bodies of Elasmobranchs correspond precisely in physiological action to the medulla of mammalian suprarenal.

With extracts of the interrenal body of Elasmobranchs, partial effects only in the same direction are obtained. Fig. 2 shows a tracing obtained by injecting the same amount of "interrenal" as

Fig. 2.



was used of "segmental suprarenal" in fig. 1. There is a striking difference between the two results.

With extracts of the suprarenals of Teleosts no effects whatever are produced. Fig. 3 shows a blood-pressure curve unaffected by the injection of extract of suprarenals of eels to the same amount as was previously employed in other experiments. Subsequent employment of mammalian suprarenal extract in the same experiment produced its proper effect.

Thus we have negative evidence of a physiological character that the interrenal body of Elasmobranchs corresponds to the "corpuscles of Stannius," the known suprarenal bodies in Teleosts, a conclusion both Diamare (5) and I myself (5, 17) had arrived at from anatomical and histological considerations.

FIG. 3.



Control experiments with extracts of "head-kidney" and muscle of fishes always gave negative results.

Perfusion Experiments.—These have been performed upon large toads. A cannula was tied into one aorta, the ligature also including the other. A snip was made in the sinus venosus, and then normal saline (0·6—0·9 per cent.) or Ringer's circulating fluid was perfused through the blood-vessels from a funnel placed about 9 to 12 inches above the animal. The toad was held in a suitable holder, and the fluid which ran through was collected in graduated cylinders and measured at the end of every five minutes. I have performed eleven of these experiment with practically uniform results. It will only be necessary to give the details of two typical ones.

In other experiments an extract made from the lymphatic "head-kidney" of different Teleosts was perfused, and also one made from muscle of various fishes. Both these always gave negative results when properly filtered.

In Experiment 1 it will be noticed that the interrenal gives a very definite result, though not so marked as that of the medullary glands. This point will be referred to again immediately. It is clear from Experiment 2 that extracts of the suprarenal bodies of Teleosts are inactive as regards the flow of fluid through the blood-vessels of a toad.

Exp. 1.—Material taken from a fair-sized Specimen of *Galeus canis*.

Large *Bufo vulgaris*. Fluid had been running about 30 minutes, pressure 25 cm.

Time.	Outflow in c.c.		Time.	Outflow in c.c.	
4·40	55	Normal saline.	5·35	32·0	"Paired segmental" suprarenal.
4·45	50		5·40	50·0	
4·50	50		5·45	20·0	
4·55	50	"Interrenal."	5·50	2·5	
5·0	33		5·55	1·5	
5·5	11		6·0	2·5	Normal saline.
5·10	5		6·5	1·5	
5·15	6		6·10	4·5	
5·20	6		6·15	0·5	
5·25	7		6·20	0·5	
5·30	24	Normal saline.	6·25	0·5	

Flow does not return to normal, although a much longer time is allowed to elapse.

Exp. 2.—Material from *Gadus æglefinus*, *Molva vulgaris*, and *Pleuronectes platessa*.

Large *Bufo vulgaris*. Fluid had been running about 45 minutes, pressure 30 cm.

Time.	Outflow in c.c.		Time.	Outflow in c.c.	
1·45	58	Normal saline.	2·0	90	
1·50	95		2·5	85	Fresh saline.
1·55	90	Suprarenal extract from *Gadus æglefinus*, then immediately from other two.	2·10	85	
			2·15	93	
			2·20	95	
			2·25	94	
			2·30	95	

There seems no reason to doubt that the active material in the "medullary glands" of Elasmobranch fishes is the same as that in the medulla of mammalian suprarenal, but so little of the material is procurable for chemical analysis that I have found it impossible to compare its reactions with that obtained from the higher animals. It is worth noting, however, that the extract in water or alcohol is of a pale brown tint, exactly resembling that of the medulla of mammals.* But that this brown pigment is not the active substance

* These glands become dark brown when treated with a solution of bichromate

is clear from the fact that when the material (in spirit) is kept for some time, this brown colour increases in intensity, although the extract may become quite inactive. I have found extracts in 80 per cent. alcohol become quite inactive after a lapse of two months.* The brown coloration appears to be the result of the oxidation of a chromogen previously existing in the extract.

It will have been observed that in the foregoing experiments the extract of the interrenal body produced a certain effect upon blood-pressure; this I believe can be entirely explained as the result of more or less admixture with "medullary glands." The latter are to be found close by the side of the former, so that it is practically impossible to remove it without some of them adhering. In my later experiments I tried to avoid this contamination by very careful removal of the interrenal, and, although I have not succeeded by this means in getting an extract of the interrenal quite inactive, yet I find that the more carefully it is removed, the less effect is produced by an injection of its extract. Thus I found the rise of blood-pressure due to interrenal to be much less when I had changed knife and forceps after removal of the medullary bodies, so as to avoid conveyance of the active principle in this manner.

Another explanation might be urged. The Polish physiologists find that a slight rise of blood-pressure is produced by an extract obtained from cortex only, and if this were the case it is conceivable that my results, in the case of the interrenal, are due to this slight specific action. But, when the greatest care is taken to avoid contamination, Oliver and Schäfer find the cortex quite inactive. Besides, the "cortical" glands of Teleosts give no effect and there can be little doubt that these are strictly homologous to the interrenal of Elasmobranchs.

The morphological conclusion that the two kinds of gland in Elasmobranch fishes really correspond to the cortex and medulla of mammalian suprarenal is not without its physiological significance. The cortex is always much more abundant than the medulla in mammals, resembles a secreting gland in many points of structure, and has possibly a function distinct from the medulla. The anatomical union of the two constituent portions may be in some sense accidental.

Conclusions.

1. The suprarenal capsule of the mammalia corresponds to two distinct glands in Elasmobranch fishes, the medulla corresponding *in structure and function* to the "paired segmental" suprarenal

of potassium, and this constitutes a ready means of displaying them in a dissection (Semper). This test probably applies to "medulla" throughout the vertebrata.

* When there is free access of air.

bodies ("medullary glands" they may be called), while the cortex corresponds to the interrenal body.

2. In Teleosts the medulla appears to be unrepresented, the known suprarenal bodies ("corpuscles of Stannius") consisting entirely of cortical substance, and corresponding in structure, and most probably in function, to the interrenal body of Elasmobranchs.

3. The same is most probably true of Ganoids, although I am guided here solely by histological evidence; I have not been able to obtain sufficient and suitable material for physiological investigation.

Thus it appears from these researches that two primary groups of the class *Pisces* (Teleosts and Ganoids) have no "medulla" but only "cortex."* So far as I know, the only piece of work published on the physiology of the suprarenal capsules in fishes is that of Pettit (12). This observer has made out a true physiological compensatory hypertrophy of one suprarenal in the eel after the other one has been removed. This renders it probable (what indeed was suggested by histological appearances) that this "cortical gland" has a secreting function. Pettit looks upon this organ in the eel as the fundamental type of the suprarenal capsule, but it appears to me much more probable that it represents cortex alone.

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* This fact would seem to suggest that the cortex may be more important than the medulla, for, whereas in certain vertebrates the medulla can be dispensed with, the cortex is universally present.

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"The Origin and Destination of certain Afferent and Efferent Tracts in the Medulla Oblongata." By J. S. RISIEN RUSSELL, M.D., M.R.C.P., Research Scholar to the British Medical Association, Senior Assistant Physician to the Metropolitan Hospital, and Pathologist to the National Hospital for the Paralysed and Epileptic, Queen Square, London. Communicated by Professor VICTOR HORSLEY, F.R.S. Received February 18,—Read March 11, 1897.

(Abstract.)

In attempting to arrive at definite conclusions with regard to the origin and destination of some of the afferent and efferent tracts which exist in the medulla oblongata, the following experimental procedures were adopted.

1. Section or destruction of the lateral region of the medulla between the ascending root of the fifth nerve and the inferior olive.
2. Division of the restiform body.
3. Division of the direct sensory cerebellar tract of Edinger.
4. Severance of Deiters' nucleus from its connections with the medulla.
5. Section of the posterior columns and their nuclei in the medulla.

The first of these procedures was followed by degeneration of two efferent tracts which could be traced throughout the whole length of the spinal cord, the one occupying the antero-lateral region, and the other being closely related to the crossed pyramidal tract, as regards position. In addition to this, afferent fibres degenerate from the seat of lesion, some of which pass to the cerebellum by way of the restiform body, others course through the medulla and pons external to the superior olive and on the ventral side of the emergent roots of the seventh and fifth cranial nerves to eventually reach the middle lobe of the cerebellum by way of its anterior peduncle. Situated internally to the tract just described is another bundle of more scattered fibres, close to the outer end of the fillet, with the fibres of which system they remain intimately associated in their further course towards the mesencephalon, and can be traced to the region of the anterior corpora quadrigemina. Owing to destruction

of a large number of the arciform fibres by the lesion, degenerated fibres were traced to the opposite inter-olivary layer, and well marked degeneration of the opposite fillet, consequent on this, could be traced to the region of the corpora quadrigemina. So too, fibres passing to the inferior olives were interrupted by the lesion, and degenerated in consequence.

Division of the restiform body, unattended with accidental complication, resulted in degeneration of afferent fibres to the cerebellum, and of efferent fibres to the formatio-reticularis, and the inferior olives; no tract in the spinal cord being found degenerated after this lesion.

In dealing with the results of section of the direct sensory cerebellar tract it is insisted that this structure ought to be regarded as totally distinct from the restiform body, and it is further pointed out that it is most difficult to sever this connection of the cerebellum without injury to Deiters' nucleus, which is imbedded in the fibres of this tract.

No efferent tracts were found degenerated other than those consequent on injury to Deiters' nucleus, and no afferent fibres were found degenerating from this nucleus to the nucleus globosus in the cerebellum.

Severance of the nucleus of Deiters from its connections with the medulla resulted in degeneration of an efferent tract passing through the formatio-reticularis of the same side of the medulla, to form a well marked antero-lateral tract on the same side of the spinal cord, extending to the lumbo-sacral region. Other degenerated fibres pass to both posterior longitudinal bundles, and others beyond these structures to terminate in the opposite formatio-reticularis. Of those which enter the posterior longitudinal bundles, some have a caudal course and reach the anterior columns of the spinal cord, on both sides, to terminate in the lower cervical region; while others pass cephalwards and can be traced to the region of the quadrigeminal bodies, where they appear to terminate. Some degenerated fibres pass from the seat of lesion across the raphe to the region of the opposite superior olive, some of them appearing to enter this structure, while others become intimately associated with the opposite fillet, and pass with this structure to the region of the anterior corpora quadrigemina.

Section of the posterior columns and their nuclei in the medulla was performed with a view to control certain of the other experiments, but it was found that beyond degeneration of fibres passing to the cerebellum in the restiform body, and of the arcuate fibres to the opposite interolivary stratum, with consequent degeneration of the opposite fillet, none of the degenerations previously described in connection with the other lesions were met with.

In view of certain recent experimental observations responsible for calling into question the existence of a direct anterior pyramidal tract, evidence is adduced in support of the existence of this structure; and the various direct efferent tracts met with in the spinal cord are fully discussed.

Among the conclusions based on the results of this experimental inquiry, the following are to be found:—

1. The descending antero-lateral tract, which degenerates in the spinal cord after a lesion of the lateral region of the medulla, is probably the same as that met with after injury to Deiters' nucleus, and is probably identical with that described by Marchi as degenerating after lesions of the cerebellum, Mott after injury to ground fibres and some of the cranial nuclei, and Biedl after section of the restiform body, its real source of origin being Deiters' nucleus, as contended by Ferrier and Turner.

2. The degenerated fibres which reach the anterior columns of the upper part of the spinal cord through the posterior longitudinal bundles, after a lesion of Deiters' nucleus, are quite distinct from the above tract, and probably belong to some system of internuncial fibres similar to those traced by Boyce to the anterior columns of the spinal cord, by way of the posterior longitudinal bundles, after hemisection of the mesencephalon.

3. The direct descending tract of degenerated fibres met with in the spinal cord in close relationship to the fibres of the crossed pyramidal tract, after a lesion of the lateral region of the medulla, is probably identical with a similar tract described by Boyce after hemisection of the mesencephalon, and by Biedl after section of the restiform body and ascending root of the fifth nerve; the proximity of Boyce's columnar tract to the latter structure accounting for its probable inclusion in Biedl's lesion.

4. Fibres derived from the restiform body proper, and degenerating caudalwards after section of this structure, do not form an efferent tract in the spinal cord.

5. There is no evidence that the "direct sensory cerebellar tract" of Edinger is composed of afferent fibres, but, on the contrary, it appears to be an efferent tract from the nucleus globosus to the nucleus of Deiters, as suggested by Ferrier and Turner.

6. One of the afferent tracts met with after a lesion of the lateral region of the medulla corresponds so closely in position and distribution with the afferent antero-lateral tract of Gowers that it is highly probable that they are identical, and accordingly that the observations of Mott and of Auerbach are correct in this connexion.

7. Another efferent tract which degenerates after the lesion of the lateral region of the medulla, and which is related to the fillet in its course to the region of the quadrigeminal bodies, is probably the

same as that described by Mott as a distinct part of the antero-lateral tract of Gowers, and is certainly no artifact, as has been suggested by Patrick.

"On the Orientation of Greek Temples and the Dates of their Foundation derived from Astronomical Considerations, being a Supplement to a Paper published in the 'Transactions of the Royal Society,' in 1893." By F. C. PENROSE, F.R.S. Received February 24,—Read March 11, 1897.

(Abstract.)

The paper first states briefly the link which connects this branch of archæology with astronomy, namely thus:—The great temple functions were celebrated at early dawn, the principal annual function being on the day when the sun, rising above the visible horizon, shone along the axis of the temple, through the eastern door, upon the Statue of the Deity in the sanctuary, the axis of the temple having been so directed at its foundation—an institution which seems to have originated in Egypt, but which was, from the earliest times, adopted in Greece. But as some time was required by the priests for preparation for the ceremony, in the absence of clocks, the rising or setting of an heliacal star would have to be observed from the sanctuary to give warning of the sun's approach; and it would be the case, roughly, that when such star could be just seen either at its appearing in the east, or just before its disappearance in the west, it would give about an hour's time for preparation. The data for the calculation are: the latitude, the amplitude given by the direction of the temple's axis and the altitude of the visible horizon. From these the sun's right ascension and declination are deduced, then the place of a star having been found suitable for acting as a warning star—which could not have been always possible without original contrivance—the precessional movement of such star will then give the period which has elapsed since it was in accurate heliacal correspondence with the sun.

In the paper are recorded some observations of the heliacal visibility of stars chiefly before sunrise, and the different angles of solar depression which seem to me to be required for stars of different magnitudes, and particularly as to the visibility of the Pleiades in twilight.

Then the elements of orientation of a number of temples are given, viz., four from Athens, which were not included in the former series, a new determination of the ancient Heræum near Argos, the former having been taken before the site was completely cleared; of the temple of Apollo at Delphi, of which the site is very

peculiar; but the temple is shown to have apparently followed the general rule, and that when rebuilt about three hundred years after the first foundation, the orientation was corrected so as to follow the precessional movement of the star.

In the examples of Greek remains in Calabria, of which six examples are given, are two cases of orientation to the winter solstice, and an instance is afterwards given from Pompeii (originally a Greek city) of orientation to the summer solstice—of these extremes I have found no examples in Greece itself. One of the examples of the winter solstice, from the ancient city of the Locri, is remarkable as being taken from foundations lying underneath those of a later temple with very different orientation and a different heliacal star. The winter solstice temples were warned by the setting, and the summer solstice temple by the rising, of the same star, β Geminorum.

The dates of the earliest of the Italian temples follow, at an interval of nearly nine hundred years, that of the foundation of the earliest of those in Greece. The temples in Sicily are rather later still, which would be the natural development, and are also in remarkable accordance with what is known by historical tradition of the Greek civilization of the island.

From Thucydides B. VI are derived the dates at which Hellenic colonies founded, or occupied, the following cities:—

Syracuse	B.C.	734
Selinus		629
Agrigentum		582

And it may be inferred from the same authority that the foundation of Segeste preceded the above given dates.

The orientation dates which I have derived for temples in these cities are—

Segeste	B.C.	830
Syracuse		815
		695
		450
Selinus		870
		610
		550

There are also at Selinus four other temples, of which I only obtained approximate orientations, sufficient, however, to show me that their dates would not be anterior to the seventh century B.C.

Agrigentum	B.C.	820
		470
		450
		430
		400

It is known from Diodorus that the great temple-building period at Agrigentum commenced after 480 B.C., and further, the temple the fourth on the list was incomplete as respects its roof in 406, and was never finished afterwards. It was a huge temple, and the orientation date for the foundation has great architectural probability. After a shock to its prosperity in 406, the city partially recovered, and six years afterwards may have been able to found the small temple with which I have closed the above list. Also the orientation date of the temple at Paestum coincides exactly with a passage in Herodotus in which, although he does not mention the temple, he speaks of the celebrity of a Posidonian architect.

"A Study of the Phenomena and Causation of Heat-contraction of Skeletal Muscle." By T. G. BRODIE, M.D., and S. W. F. RICHARDSON, M.B., B.S. Communicated by W. D. HALLIBURTON, F.R.S. Received March 2,—Read March 11, 1897.

(From the Physiological Laboratory of St. Thomas' Hospital.)

(Abstract.)

In carrying out some experiments to determine the changes in length of a muscle when subjected to varying loads and temperatures, we have paid particular attention to those changes produced from the onset of heat-contraction up to a higher temperature. We have, for the particular study of these changes, always employed a sartorius preparation of a frog immersed in diluted defibrinated blood, which was gradually heated in a water bath. The changes in length were recorded photographically. The necessary tension was applied by means of a very weak spiral spring.

If the load be low—we have usually employed one below 1 gram—the record shows four separate contractions. These are:—

(1) A contraction commencing at about 32° C., and ceasing at about 40° C.

(2) A second commencing at about 46° C., and ceasing soon after 50° C.

(3) A third commencing at about 56° C., and ceasing soon after 60° C.

(4) A fourth commencing at about 63° C., and extending up to 75° C.

If we contrast the temperatures of these contractions with those given as the points of coagulation of the proteids of frog's muscle plasma by v. Fürth, we find that the first three agree in all points

with the proteids termed by v. Fürth soluble myogen-fibrin, myosin, and myogen respectively. The fourth corresponds with the temperature at which white fibrous and yellow elastic tissue contract when heated.

We have concluded from this and many other facts that the contractions produced by heat are at basis caused by the heat-coagulation of the different proteids.

We have obtained corresponding results with mammalian muscle; here the contraction at 32—40° does not occur, and soluble myogen-fibrin is absent from the muscle plasma.

"Some Experiments with Cathode Rays." By A. A. C. Swinton. Communicated by Lord Kelvin, F.R.S. Received February 27,—Read March 11, 1897.

The extensive employment of the focus form of Crookes' tubes as the most efficient known means of generating X-rays, has rendered advisable the more complete investigation of the cathode ray discharge in tubes of this description.

Hitherto, the usual method of investigating the characteristics of a cathode ray discharge apart from its mechanical properties, and beyond what is visible to the unassisted eye, has been by allowing the rays to fall upon a screen of some brightly fluorescent material, such as glasses of various descriptions, or screens covered with fluorescent salts. With all of these the maximum amount of fluorescence appears to be produced by such comparatively weak cathode rays, that in some cases the special effects produced by the more powerful rays seem to be more or less entirely masked, while the well-known phenomenon of the fatigue of fluorescent substances, when exposed to the more active rays, conduces to the same result.

Surface Luminescence of Carbon when exposed to Cathode Rays.

I have found in some cases that by replacing the usual screen, made of or covered with fluorescent material, by one of ordinary electric-light carbon, much appears which was previously invisible. When a concentrated stream of powerful cathode rays are focussed upon a surface of carbon in this manner, a very brilliant and distinctly defined luminescent spot appears on the surface of the carbon at the point of impact of the rays, the remainder of the carbon remaining black. This luminescent spot seems to have a very close relation to the fluorescent spots on glass and on other fluorescent materials under similar influence. The effect is evidently a purely surface effect, as when the cathode stream is rapidly deflected by

means of a magnet, the luminescent spot on the carbon moves with no perceptible lag. Further, though, as is also the case with glass, the whole of the carbon becomes gradually heated to a considerable extent if much power be employed for a long period of time, these luminescent spots are instantaneously produced on carbon of very considerable brilliancy with but a comparatively low power. Again, just as glass is known to become fatigued under the influence of cathode rays, so that after a time it refuses to fluoresce so brightly as before, so carbon is similarly fatigued, though only after having been very strongly acted upon. Carbon, like glass, also recovers its property of giving a surface luminescence to some extent, though it does not seem to entirely recover, at any rate at all rapidly.

That the rays which produce the luminescence of the carbon are the same rays that cause fluorescence of the glass can be proved by deflecting the rays from the carbon on to the glass by means of a magnet.

As it is exceedingly difficult, if not impossible, to obtain carbon plates which do not contain hydrocarbons and other volatile matter which are rapidly given off and reduce the vacuum very quickly when the carbon becomes at all heated, it is necessary to keep the tube connected to the mercury pump, so that the vacuum can be restored after each experiment. This arrangement was followed in all the experiments described below, except where specific mention is made to the contrary.

Apparent form of the Cathode Ray Discharge in a Focus Tube.

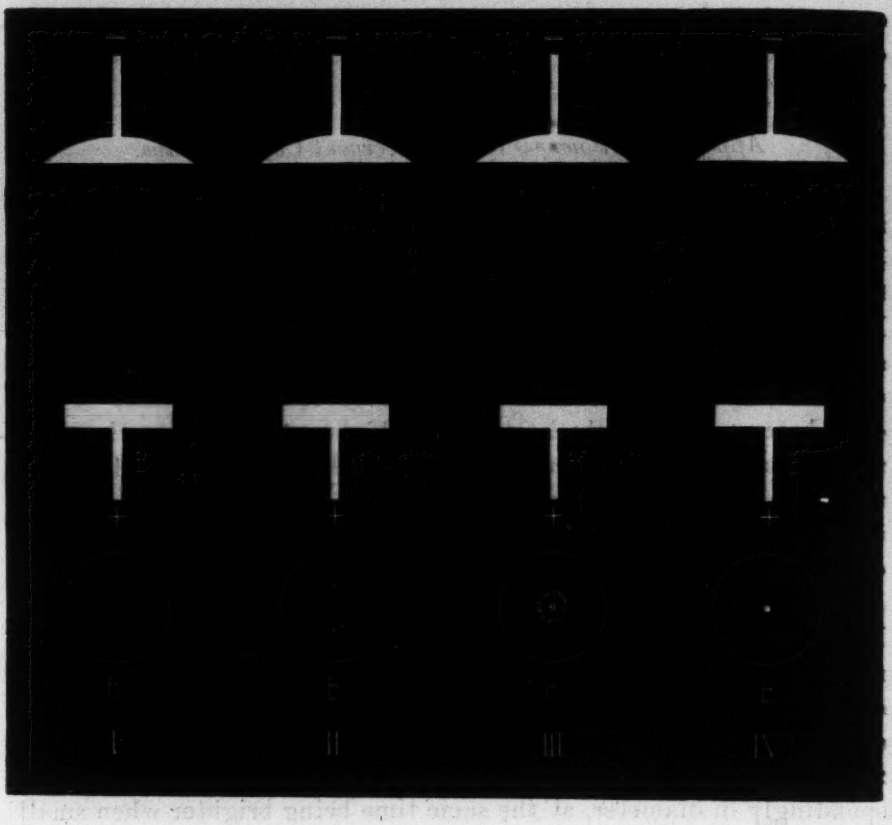
As is well known, in tubes of the ordinary focus type with a single spherical concave cathode, the rays coming off normally to the cathode surface appear to converge in more or less of a cone to a focus, and if the vacuum be not too high, to diverge again immediately in another cone upon the other side of the focus. At higher vacua the rays, after passing the focus, do not appear to diverge again at once, but seem to form themselves into a description of thread which connects the convergent and divergent cones, and is longer or shorter according as the vacuum is higher or lower. The angle of the divergent cone appears, however, to be always proportional to that of the convergent cone. The focus, or perhaps more correctly, the point at which this thread commences, seems always to be more distant from the cathode than the centre of curvature of the latter, but the variation in this respect seems to be less and less the higher the exhaustion. This is no doubt due to the mutual repulsion of the rays, and accords with the assumption that the rays consist of charged particles, which travel more and more rapidly the higher the exhaustion. Probably for the same reason, cathodes that are

only slightly concave, focus further in proportion beyond their centres of curvature than do deeply concave cathodes, for the same vacuum.

Apparent Hollowness of the Divergent Cone of Rays.

When the divergent cone is thrown upon a thin platinum disc, as in the ordinary focus tube, and sufficient electric power—say, from a 10-in. Ruhmkorff coil—is employed, the platinum quickly attains to a red heat. With platinum, either the whole disc becomes uniformly heated, or in the event of the diameter of the cone of rays where it strikes the platinum being small compared with the area of the platinum, that portion of the platinum covered by the base of the cone becomes uniformly heated to a higher temperature than the remainder. This is as much as can usually be seen with platinum, though rather more is sometimes visible with aluminium; but if instead of either metal the disc is made of ordinary electric-light carbon, I have found that the luminescent portion of the carbon, instead of comprising the whole disc, or consisting of a uniformly heated circle, will in some cases take the shape of a brilliantly luminescent and apparently white-hot ring, with a well-defined dark, and seemingly quite cold, interior. As the dimensions of the cone of rays are increased or decreased by decreasing or increasing the vacuum, the luminescent ring will be found to increase or decrease correspondingly in diameter, at the same time being brighter when small than when large. Further, when the ring is very small it will usually have a very brightly luminescent central spot, with a dark intervening portion between this spot and the ring, and when the vacuum is further increased the ring will gradually close in upon the spot until only the latter remains.

Figs. 1, 2, 3, and 4 show diagrammatically these hollow effects, as produced by spherical aluminium cathodes, 1·125 in. diameter and 0·708 in. radius of curvature, for four different degrees of vacuum, 1 being the lowest and 4 the highest exhaustion. The upper portion of each of these figures represents the general appearance of the cathode discharge between the spherical concave aluminium cathode C at the top, and the carbon anti-cathode B at the bottom, as accurately as it is possible to represent evanescent coloured appearances in monochrome. The other appearances, due to the dark space and fluorescence of the glass, are omitted for the sake of simplicity. Beneath each of the elevational views of the cathode discharge will be found a plan view of the carbon anti-cathode, showing for each condition of vacuum the effect of the cathode discharge upon the carbon anti-cathode, in forming a brightly luminescent hollow ring, gradually decreasing in diameter as the vacuum is increased, until it centres on a point, as already mentioned.



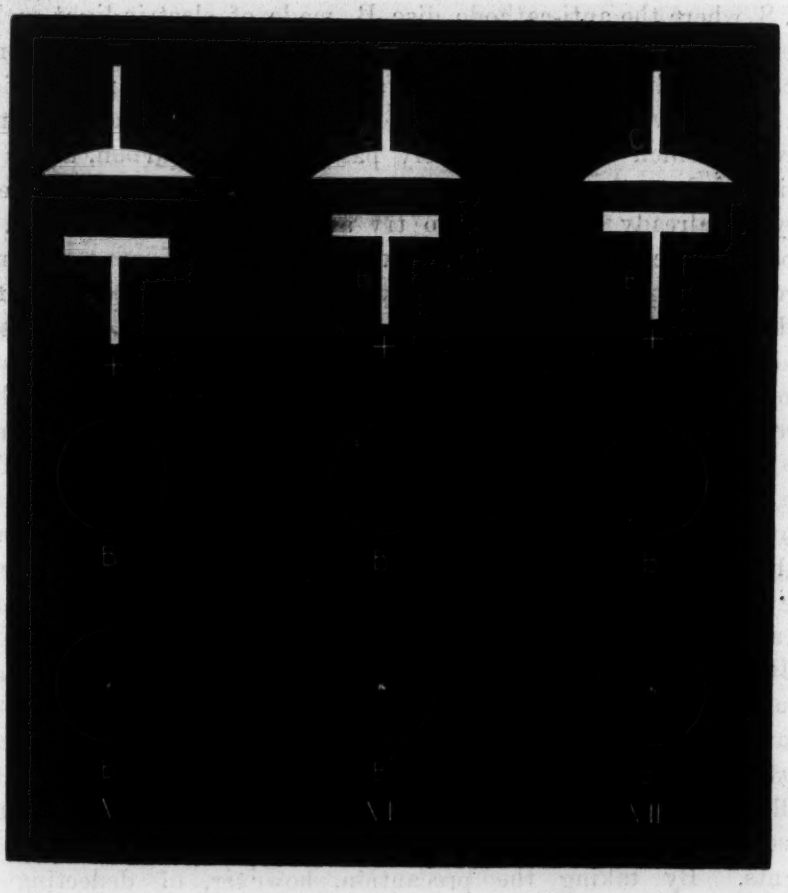
It may further be remarked that the diameter of the luminescent ring may be increased or diminished, or finally reduced to a point, without altering the degree of vacuum, by moving the anti-cathode away from or towards or finally into the focus of the cathode stream, the appearance of the ring in each of these cases being practically similar to those shown in the figures for a uniform distance with varying vacuum. When the anti-cathode surface is not at right angles to the line of the discharge, the ring, in place of being circular, takes the proper form of a conic section. The holding of a magnet near the tube distorts the ring from a circular shape and moves its position on the carbon.

From these experiments it appears that the diverging cone of cathode rays acts as though it were not of uniform density throughout its section, but, at any rate in some instances, as if it were completely hollow. This fact does not appear to have previously been noted.

Apparent Hollowness of the Convergent Cone of Rays.

The apparent hollowness of the divergent cathode ray being thus established, it was thought desirable to ascertain whether the same

condition of affairs exists in the converging beam of rays between the cathode and the focus. Owing to the well-known difficulty of getting any discharge to pass when the distance between the electrodes is less than the thickness of the dark space, and to the disturbing effect which the anti-cathode screen is found to have when brought within the focus of the cathode, especially with high vacua, this question was found much more difficult to decide than that of the hollowness of the divergent cone. However, that the convergent cone also acts under certain circumstances as though it were almost completely hollow, and acts generally as if it had a considerable tendency towards hollowness at low vacua, was also finally fully determined. The lower portions of figs. 5, 6, and 7 show the bright



ring appearance upon the carbon anti-cathode at two different degrees of exhaustion, B being higher vacua than B′, and with the anti-cathode at the three different distances from the cathode within the focus of the latter, as shown in the upper part of each figure. As will be observed in this case, the degree of vacuum is found not to appreciably affect the dimensions of the figure, though it should

be stated that the vacua in each case were comparatively low, as vacua as high as those employed when the anti-cathode was outside the focus, gave no results at all. As will be seen, however, the diameter of the luminescent ring is affected by the degree of proximity of the anti-cathode to the cathode, being larger when the distance is small than when it is great; while in every case there is a decided tendency towards hollowness, though usually with some slight internal luminescence and with a bright central spot, while in one case, when the anti-cathode was very close to the cathode and the vacuum was comparatively high, the ring is seen completely hollow, and there is no central spot.

A convenient form of tube for showing the apparent hollowness of both the divergent and convergent cone of cathode rays is shown in fig. 8, where the anti-cathode disc B, made of electric-light carbon, is supported upon a small carrier which slides upon the bottom of the tube, and is connected to the anode terminal D by means of two aluminium wires, each of which have a ring at their extremity through which they respectively pass. As the carbon, under the action of the cathode rays, gives off hydrocarbon vapour, it is necessary, as already mentioned, to try all these experiments with the tube connected to the mercury pump; but with this connection made through a slightly flexible mercury joint it is possible by inclining and gently tapping the tube to bring the anti-cathode to any desired position either near or far away from the cathode. For experiments upon the divergent cone, it is not necessary that the anti-cathode screen should be connected to the anode terminal, and, consequently, the sliding aluminium wires inside the tube are not required. They are, however, necessary when observations are to be made on the convergent cone between the cathode and the focus, as the anti-cathode screen when placed within the focus must be connected to the anode, or it appears to get negatively charged and acts itself as an additional cathode, throwing cathode rays in all directions.

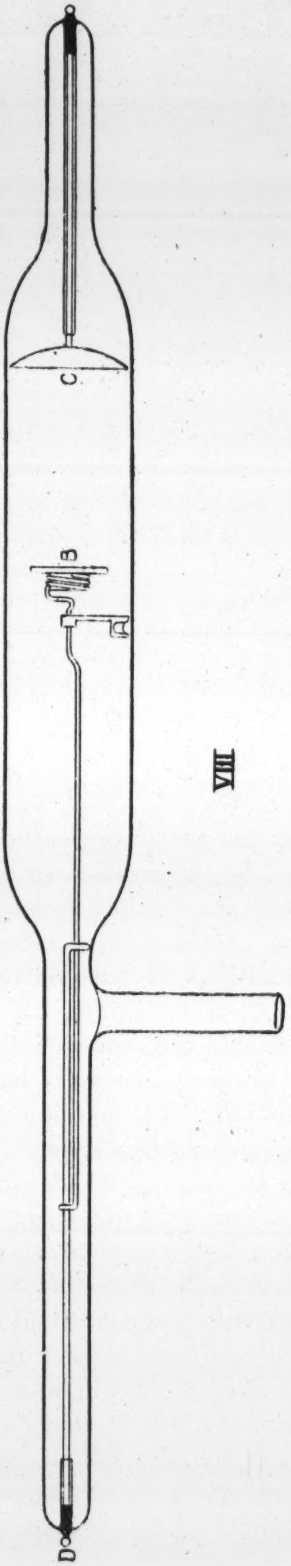
It may here be mentioned that the fatigue of the carbon already alluded to renders necessary some precautions in carrying out the above-mentioned experiments, as otherwise the observer may be misled into thinking that a beam of cathode rays is effectively hollow when this is not the case, owing to the centre of the carbon covered by the beam having been fatigued by some previous experiments. By taking the precaution, however, of deflecting the cathode beam by means of a magnet on to various portions of the carbon screen, such errors may be avoided. It should also be noted that these hollow effects appear only to be obtained with fairly short focus cathodes, such as are usually employed in X-ray focus tubes, that is to say, with cathodes whose diameter is large as compared with their radius of curvature, so that the rays converge and diverge

rapidly to and from the focus. With comparatively flat, long focus cathodes the cones do not show any signs of being hollow, and produce a uniformly luminescent spot upon the carbon of larger or smaller diameter, according to the conditions of vacuum and the position of the screen.

For instance, while cathodes 1·125 inches diameter and 0·708 inch radius of curvature gave in the manner described distinctly hollow convergent and divergent cones, a cathode 1 inch diameter and 1·5 inches radius of curvature gave convergent and divergent cones that appeared to be uniformly solid under all conditions.

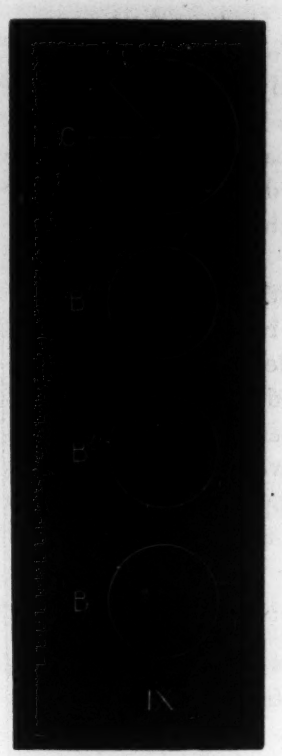
The Rays cross at the Focus with no Rotation.

In order to investigate the cathode rays in a focus tube still further, and more especially in order to discover whether the various rays from the cathode cross one another at the focus, or diverge again without crossing, and also in order to discover whether there is any twist or rotation of the rays, similar to what has been observed in the case of rays focussed by magnetism,* a tube was constructed similar to that used in the previous experiments, with a carbon anti-cathode which was also the anode, fixed at the opposite side of the focus from the cathode, with the focus about equally distant between it and the cathode. The peculiarity of this tube consisted in the fact that a sector of the aluminium cathode, equal to one-eighth of the total area of the cathode, had been entirely removed, as shown at C, fig. 9. It was expected that on using this tube, with the proper degree of vacuum to form a well-defined ring on the anti-cathode screen, that a portion of the ring, corresponding with



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* See experiments by K. Birkeland, 'Electrical Review,' June 12, 1896.



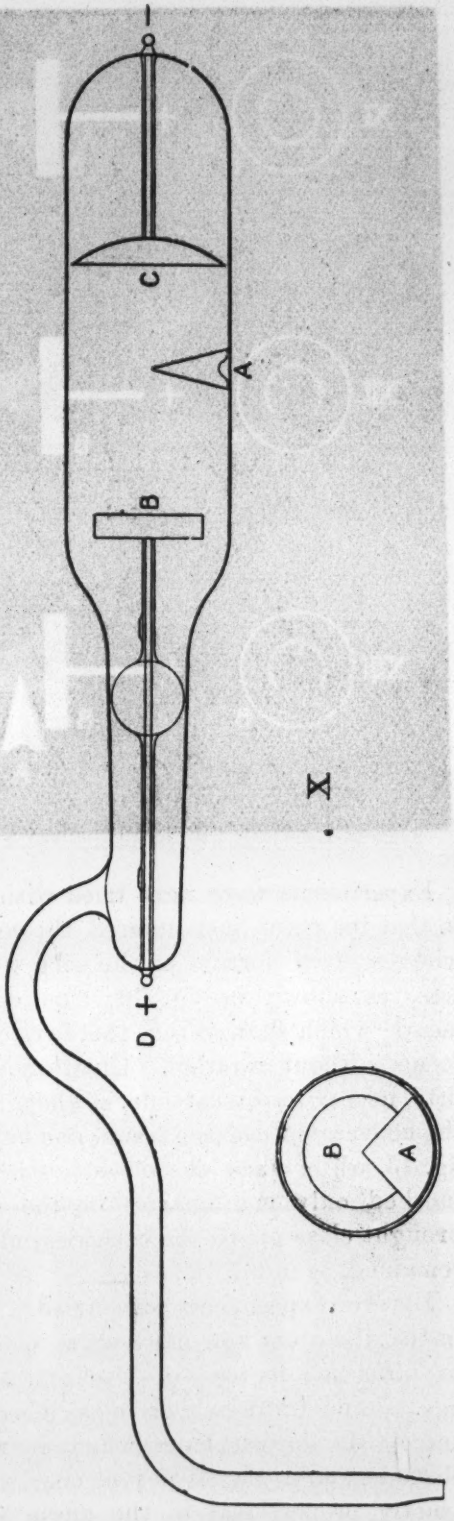
the amount of the cathode cut away, would be found wanting; and that by the position of this gap in the ring it would be possible to ascertain whether the rays crossed at the focus, and whether there was any rotation. What actually was observed is shown for three different conditions of vacuum in fig. 9, B being for the highest, and B″ for the lowest vacuum. As will be seen, the expected gap in the ring was obtained, but with the unexpected addition that the dimensions of this gap, instead of being only one-eighth of the circumference of the ring, was seven-eighths of the circumference. In fact, the amount of ring shown corresponded not with the seven-eighths of the remaining cathode surface, but with the one-eighth of the cathode that had been removed. The portion of ring that did appear was of a length corresponding exactly to the arc of the removed sector of the cathode, according to its greater or lesser nearness to the centre with different conditions of vacuum; and as the portion of ring was in each case exactly in line with the portion of cathode that had been cut away, it would appear that there is no rotation of the cathode beam as a whole, that the rays do cross at the focus; and, further, that when the hollow convergent cone is, as it were, split in this manner, some unexplained action, similar in effect to the existence of a circular surface tension, causes the gap to widen out and the

remaining portion of the ring-shaped section of the cone to contract correspondingly, without, however, altering its diameter.

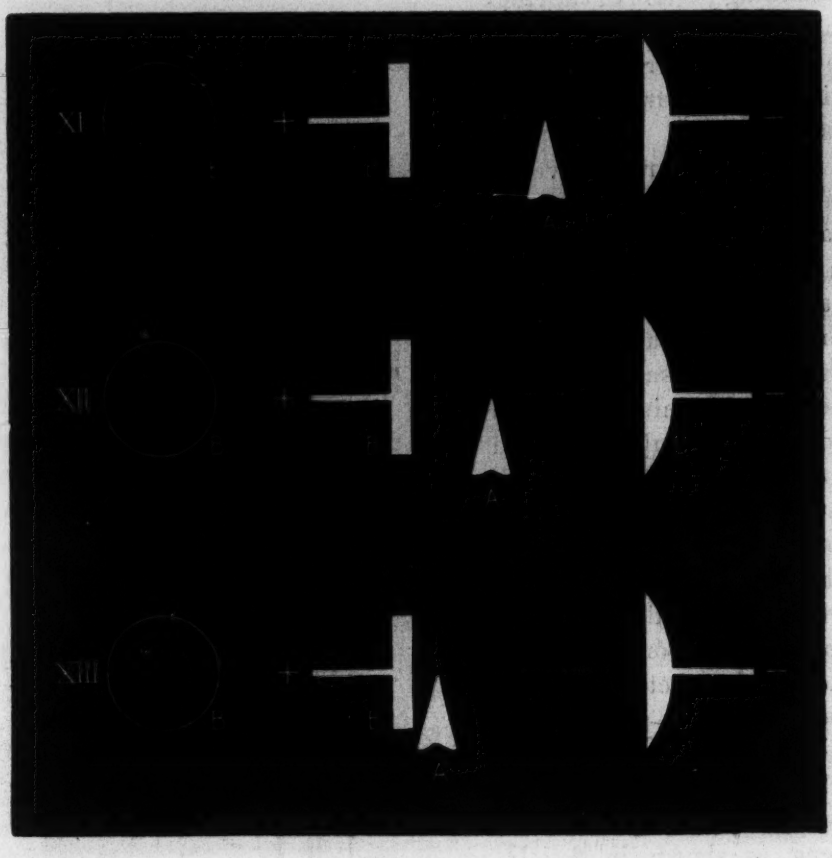
In order to further investigate the matter another tube was made, as shown in fig. 10, in which the concave cathode was complete; but the interior of the tube was furnished with a small movable piece of aluminium, A, which by shaking could be moved up and down the tube between the cathode C, and anti-cathode B, and which, while not quite reaching the centre of the tube, would fill up very nearly one quarter of the circular sectional area of the latter.

With this arrangement of tube, with the aluminium obstacle placed just at the focus, as shown in fig. 11, the point of the obstacle just missing the cathode rays, a complete ring was formed on the carbon anti-cathode. On moving the obstacle slightly into the divergent cone, exactly one quarter of the ring on the anti-cathode failed to appear, as shown in fig. 12, and on the obstacle being further moved in the same direction the result was not altered, as shown in fig. 13.

As in each of the latter two cases there was no displacement of the gap in the ring, the above showed that there is no rotation of the divergent cathode cone.

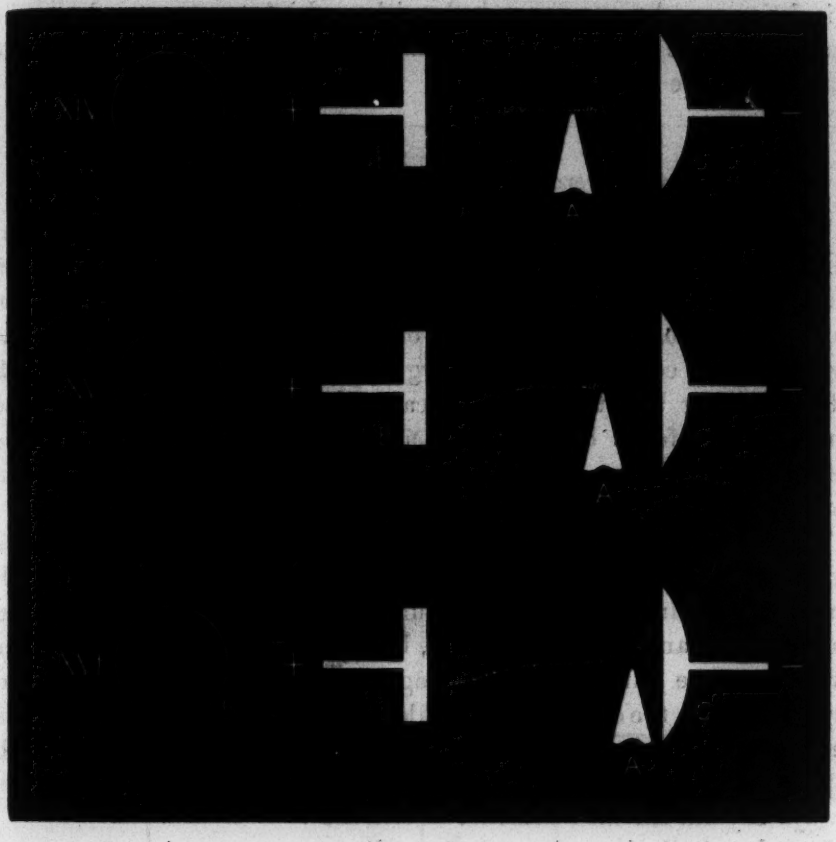


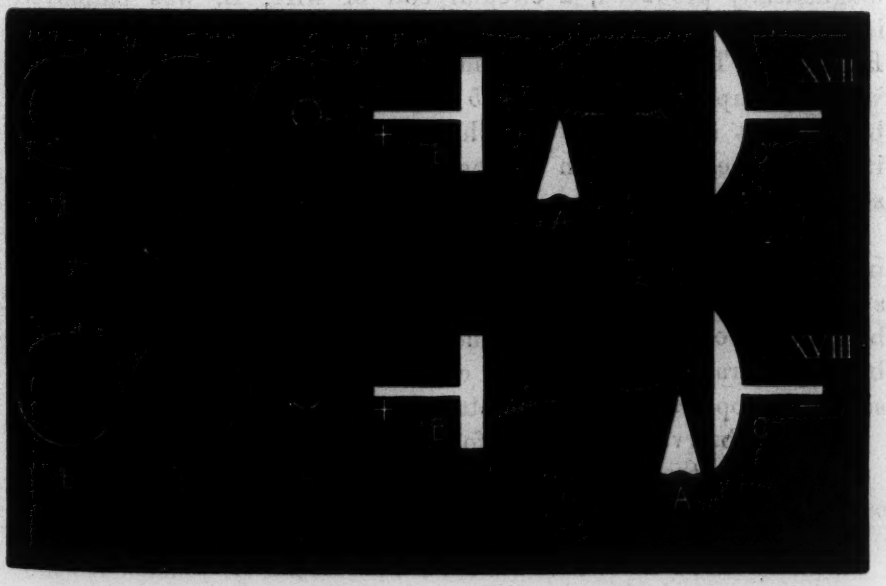
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Experiments were next tried with the aluminium obstacle, moved so that its point just entered the converging cone of cathode rays, when a small portion of the ring was cut out, but on the opposite side, as shown in fig. 14, this confirming the previous experiments, which showed that the rays cross one another's paths at the focus without rotation. Upon moving the aluminium obstacle a little nearer to the cathode, so that its point entered still further into the convergent cathode beam, one half of the ring disappeared, as in fig. 15, while when the obstacle—which, it should be remembered, blocked only one quarter of the circular area of the tube—was brought close up to the cathode, only about one quarter of the ring remained, as in fig. 16.

Further experiments were tried with the aluminium obstacle both in the divergent and convergent cones, but with the tube exhausted to different degrees of vacuum, the result being as shown in figs. 17 and 18, in which in each case B shows the highest vacuum and B″ the lowest, from which it will be observed that when the obstacle was in the divergent cone, a portion of the ring was cut off exactly proportional to the angle subtended by the sides of the obstacle; while when the obstacle was placed in the convergent





cone, a much larger proportion of the ring was cut off in each case, this being much more marked with a high vacuum when the diameter of the ring was small, than with a low vacuum when the diameter of the ring was large.

Convergent and Divergent Cones produced by Magnetic Focussing.

In order to discover whether the apparent hollowness of the convergent and divergent cones of cathode rays as above observed, when the focussing was performed by means of a spherical cathode, was in any way due to the concave form of the cathode or to the fact that the rays were converging or diverging, experiments were tried with a tube having a flat aluminium cathode, the rays being caused to converge to a focus by means of a powerful electro-magnet in the manner described by the writer in his paper on "The effects of a strong magnetic field upon electric discharges *in vacuo*." *

The arrangement is shown in fig. 19, the carbon anti-cathode screen, B, being movable, and not connected to the anode D, which was contained in an annex to the tube. By increasing or decreasing the power of an electro-magnet M, by moving it nearer to or further away from the tube, and by moving the anti-cathode screen up and down the tube, the cathode rays could be focussed on the anti-cathode screen so as to form a circle of any desired size, the focus, which appears to be exactly on the pole of the magnet, being, of course, always beyond the anti-cathode.

In order similarly to investigate a divergent cone of cathode rays magnetically produced, a circular coil of wire, E, was employed instead of the magnet in the manner recently described by Professor Fleming.† This coil, which had 72 turns of No. 18 S.W.G. size wire, was supplied with 20 to 25 amperes of current from a storage battery. It focussed the cathode rays at a point exactly central to its own plane, from which they again diverged on to the anti-cathode screen.

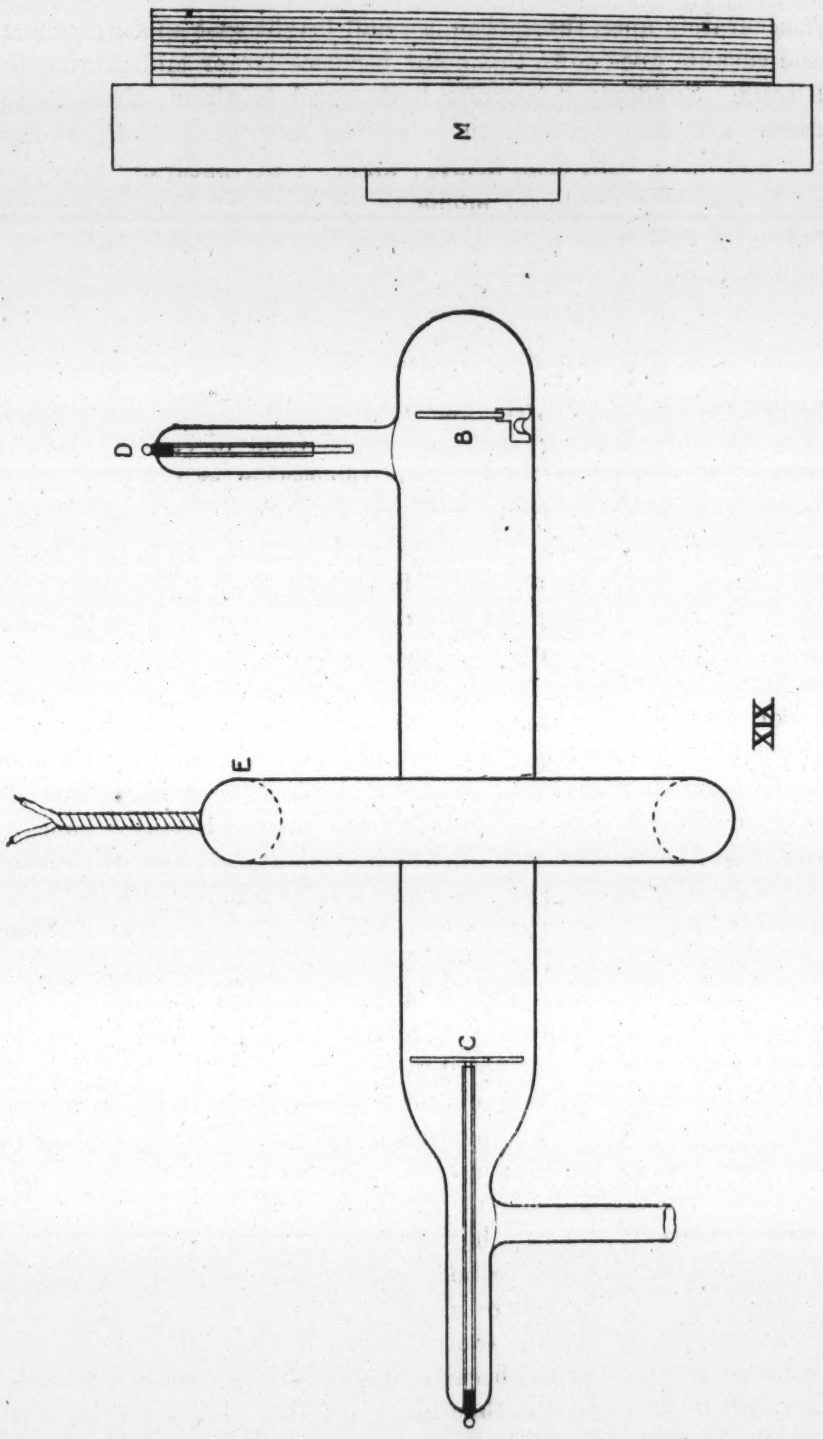
With convergent and divergent cones of rays produced magnetically in the above manner, there was no difficulty in showing that, under suitable conditions, these cones acted as if they were hollow, giving bright rings of varying sizes, sometimes with and sometimes without bright central spots, upon the carbon anti-cathode screen, exactly similar in appearance to those obtained with the concave cathode.

Further observations were as follows :—

In some instances, two concentric hollow rings were observed, especially with a low vacuum, when the magnet was suddenly turned

* 'Roy. Soc. Proc.,' 1896, vol. 60, p. 179.

† 'Electrician,' January 1, 1897.



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on or off. The rings are probably not simultaneous, but successive, but this cannot be detected with the unaided eye.

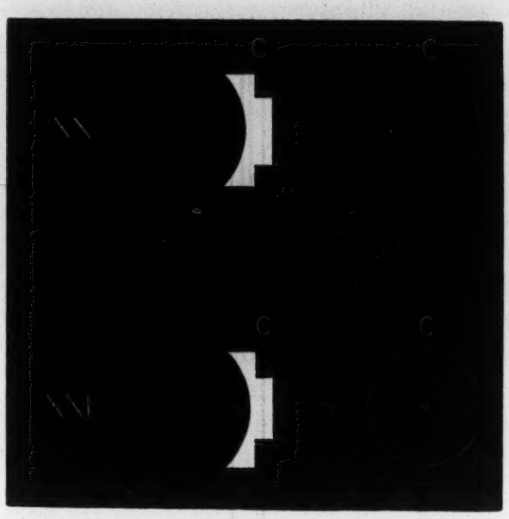
With a high vacuum, and the magnet so arranged as to focus the

rays accurately upon the carbon, a small bright spot appears at first; as the vacuum goes down this point becomes larger and fainter, but still solid. Suddenly it becomes hollow and brighter; then, as the vacuum falls still further, the ring becomes solid again, though larger and more faint than before; finally it disappears. After this stage it can be reproduced momentarily, without alteration to the vacuum, by switching the magnet on and off suddenly, when it is usually hollow, but sometimes solid.

The Convergent Cone at Higher Vacua.

As has been mentioned, the carbon anti-cathode screen was found useless for investigating the convergent cone of cathode rays at anything but a very low vacuum, by the reason of the well-known difficulty in getting any discharge to pass when the distance between the electrodes is less than the thickness of the dark space, and for the further reason that if the anti-cathode screen was not connected to the anode, it became itself negatively charged and acted as an additional cathode when brought into the space between the cathode and the focus.

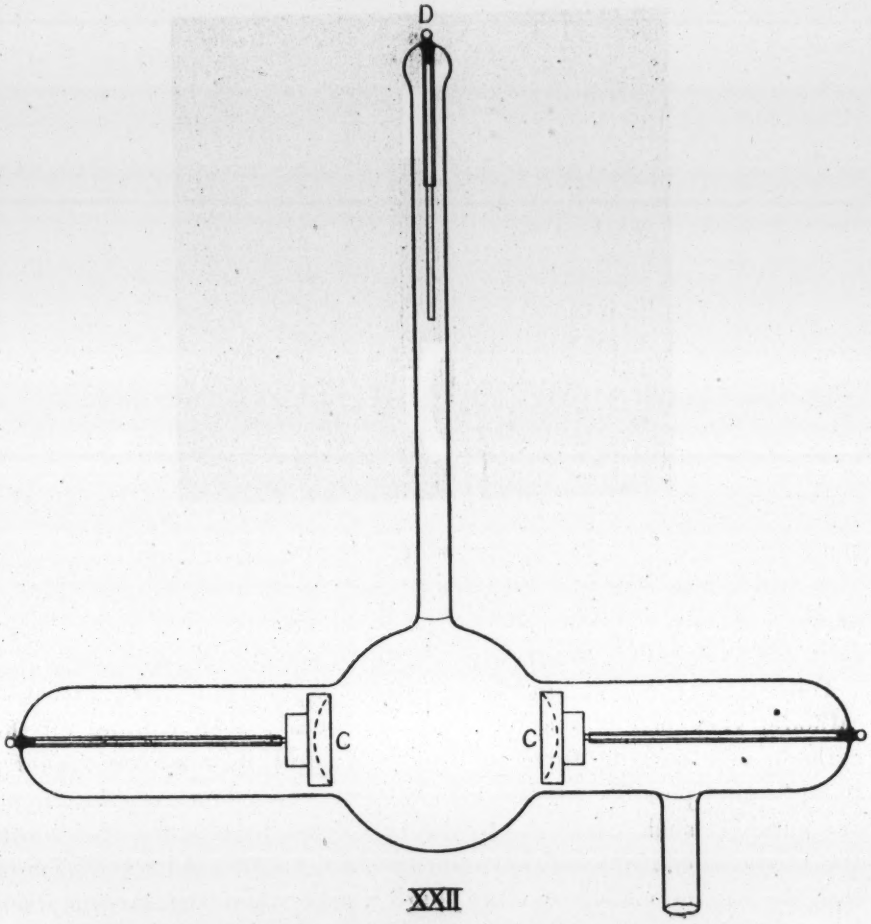
Under these circumstances, it was thought that possibly some additional information might be obtained with regard to the form of the convergent cone at high vacua, by making the concave cathode itself of carbon. A tube was therefore constructed having a concave carbon cathode, the diameter of which was 1 inch, and the radius of curvature 0·75 inch. The appearance of the cathode with this tube is shown for a fairly high vacuum in fig. 20, in which the cathode itself is shown in section, so as to let the form of the discharge be better seen. As will be observed under this condition of vacuum, which was too high to show any divergent cone, the cone of convergent rays appears to be contracted in diameter at its base, and to come off from the central portion of the cathode only, the remaining surface of the cathode being apparently inactive. This was found to be still more the case at higher vacua, as will be seen from fig. 21, which shows in a similar manner the form of the cathode discharge in a tube exhausted to a very high vacuum. In this case, as will be observed, the whole of the cathode rays appear to come off from a very small spot in the centre of the cathode. Further, that this small spot is, at any rate, the source of most, if not all, activity, was evident from the fact that it became luminescent exactly in the same manner, but in a less degree, than had previously been observed with a carbon surface upon which cathode rays were concentrated. Whether this surface luminescence of the cathode carbon at the point where the cathode rays leave it is due to the violent tearing away of particles of carbon, or to some other cause, it is difficult to say, but



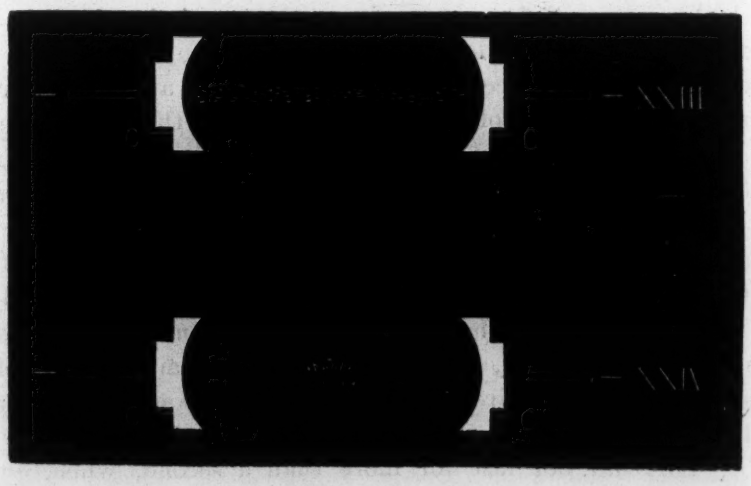
the fact that at high vacua the cathode rays come off entirely or, at any rate, almost entirely from only a very small portion of the centre of the cathode, explains the observed fact that within limits large cathodes have no advantage over small cathodes in X-ray tubes.

During the carrying out of the above experiments with a carbon cathode, very bright sparks were occasionally seen coming off the cathode and passing through the focus, and it was consequently thought that possibly by placing two concave carbon cathodes facing one another, such particles, by being caused to rebound backwards and forwards continuously between the two, might render the form of cathode stream visible at very high vacua when the stream itself becomes otherwise invisible.

With this view, a tube was made as shown in fig. 22, in which two concave carbon cathodes CC, similar to those employed in the last experiment, were placed exactly opposite one another, so that a prolongation of the focus of either one would pass through the centre of the other. The anode, D, was placed in an annex, as shown in the illustration, and the two cathodes were connected together by means of a wire outside the tube. At a very high exhaustion, this tube gave very beautiful effects, and showed clearly the form of the cathode discharge at a degree of exhaustion when it is usually in itself quite invisible. Immediately on the current being turned on and the discharge passing, a straight and thin stream of bright golden coloured particles of apparently incandescent carbon passed between small luminescent spots at the centres of each cathode, as shown in fig. 23. This did not last for more than a second, when, owing no doubt, to the rapid fall of vacuum, the appearance changed to that shown in fig. 24, and the incandescent particles of carbon could be seen passing backwards and forwards along the convergent and



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divergent cones of cathode rays, which, at the lower vacuum, proceeded from both cathodes, and spluttering in the centre, where the particles going in opposite directions collided. This appearance

lasted for some seconds, becoming gradually fainter as the vacuum fell. By re-exhausting the tube with the pump, however, the original appearance shown in fig. 23, as also the appearance shown in fig. 24 could be produced as often as desired. Apparently the particles of carbon become heated to incandescence either by the action of the cathode rays upon them while they are flying through space, or by their friction in passing through the residual gas, and possibly by their mutual collisions, for in the stage shown in fig. 24, when the cathodes themselves show no luminescence, the flying particles appear to be most intensely luminescent when in the centre of the tube. It may be mentioned that after this experiment had been repeated several times, the glass of the tube became perceptibly blackened, which, taken with the fact that a similar tube with cathodes of aluminium showed no stream of bright particles, goes to show that the particles consist of carbon torn off the surfaces of the cathodes.

The Production of X-Rays.

The tube, fig. 22, with carbon cathodes was found to produce feeble X-rays, which, when observed with a fluorescent screen, appeared to come either from the fluorescent glass of the bulb or from the travelling particles of carbon.

In order to ascertain whether it is necessary that the cathode rays should fall on solid matter in order to produce X-rays, another tube was constructed, similar in all respects to that shown in fig. 22, with the exception that the two cathodes were made of aluminium.

It was thought that with this tube the opposing streams of cathode rays might possibly produce X-rays at the point where they met. This does not, however, appear to be the case, as though this tube, when exhausted to so high an extent that the alternative spark in air leapt fully 8 inches, gave X-rays in considerable quantity, these rays appeared to come entirely from portions of the glass of the tube that were covered with green fluorescence, and not at any rate appreciably from the central point between the two cathodes, where the opposing streams of cathode rays would meet one another.

It seems, therefore, that X-rays can only be produced by cathode rays when these strike solid matter.

In conclusion I wish to mention how much I owe in carrying out these experiments to the assistance of Mr. J. C. M. Stanton and Mr. H. Tyson Wolff, who have made and exhausted all the tubes, and to whom I am also indebted for many valuable suggestions.

Report of the Kew Observatory Committee for the Year ending December 31, 1896.

The operations of The Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Observatory Committee, which is constituted as follows :—

Mr. F. Galton, *Chairman.*

Captain W. de W. Abney, C.B., R.E.	Prof. A. W. Rücker.
Prof. W. G. Adams.	Mr. R. H. Scott.
Captain E. W. Creak, R.N.	Mr. W. N. Shaw.
Prof. G. C. Foster.	Lieutenant-General R. Strachey, C.S.I.
Prof. J. Perry.	Rear Admiral W. J. L. Wharton, C.B.
The Earl of Rosse, K.P.	

On February 16 the Committee sustained a severe loss by the death of General J. T. Walker, C.B., who had been a member and constant attendant of the Committee during ten years, and whose valuable assistance was of special importance to them in furthering pendulum experiments made for geodetic purposes.

The vacancy on the Committee thus occasioned was filled by the appointment of Dr. John Perry, Professor of Mechanics and Mathematics in the Royal College of Science, South Kensington.

The work at the Observatory may be considered under the following heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the departments.
5th. Verification of instruments.
6th. Rating of Watches and Marine Chronometers.
7th. Miscellaneous.

I. Magnetic Observations.

The Magnetographs have been in constant operation throughout the year, and the usual determinations of the Scale Values were made in January.

The ordinates of the various photographic curves representing Declination, Horizontal Force, and Vertical Force were then found to be as follows:—

Declinometer: 1 inch = 0° 22′·04. 1 cm. = 0° 8′·7.

Bifilar, January 15, 1896, for 1 inch δH = 0·0280 foot grain units.
„ 1 cm. „ = 0·00051 C.G.S. units.

Balance, January 16, 1896, for 1 inch δV = 0·0276 foot grain units.
„ 1 cm. „ = 0·00050 C.G.S. units.

The magnetic curves during the past year have been quite free from any very large fluctuations. Some of the principal variations that were recorded took place on the following days:—

January 3—9, 19—20, 30—31; February 1—4, 13—14, 28—29; March 4, 26—28; April 21—25; May 2—3; June 16; September 18; October 9—12; November 7—8; and December 3—4.

The earthquake of December 17 was shown slightly on the Declination curve, but more distinctly on the Horizontal Force curve, though the disturbance on the latter curve only equalled that due to a change of 0·00004 in the Horizontal Force in the C.G.S. units.

The hourly means and diurnal inequality of the magnetic elements for 1896, for the quiet days selected by the Astronomer Royal, will be found in Appendix I.

The mean values at the noons preceding and succeeding the selected quiet days are also given, but these of course are not employed in calculating the daily means or inequalities.

The following are the mean results for the entire year:—

Mean Westerly Declination	17° 10′·8.
Mean Horizontal Force.....	0·18309 C.G.S. units.
Mean Inclination	67° 22′·3.
Mean Vertical Force	0·43924 C.G.S. units.

Observations of Absolute Declination, Horizontal Intensity, and Inclination have been made weekly, as a rule.

A "Richard" Thermograph has been in constant action in the Magnetograph Room all through the year in order that its readings may be compared with the observed readings of the Thermometer placed under the Vertical Force shade.

As in 1895, a Table of recent values of the Magnetic Elements at the Observatories whose publications are received at Kew was contributed to 'Science Progress,' appearing in the August number. A similar table, but containing more recent data, will be found in Appendix IA to the present Report.

With the consent of the Committee, an analysis of the non-cyclic effects in the Declination, Horizontal Force, and Vertical Force results for the selected "quiet days" of the six years 1890 to 1895 was drawn up and discussed by the Superintendent, and appears as the Report of 'the B. A. Committee for the Comparison and Reduction of Magnetic Observations,' Liverpool, 1896.

At the request of Professors von Bezold and Eschenhagen, the Observatory participated in a combined scheme of eye observations of the Declination and Horizontal Force at intervals of five seconds during an hour on each of the four days February 27, 28, and March 12, 13. On the two last occasions use was made of a very delicate declinometer with quartz fibre suspension, specially devised by Professor C. V. Boys, F.R.S., enthusiastic assistance being given by Messrs. Cooper and Sansom, students at the Royal College of Science, South Kensington.

Owing to the erratic behaviour of the limelight employed to illuminate the declinometer mirror, the results were unfortunately less successful than might have been desired.

The Magnetic Instruments have been studied, and a knowledge of their manipulation obtained, by Commanders Smyth and Heming and Lieutenants Monro and Somerville, of the Royal Navy.

Information as to magnetic data or experimental details has been supplied to Professor Rücker, Dr. E. Atkinson, Lieutenant-Colonel Gore (Trigonometrical Survey of India), Captain Lyons, R.E., and Captain Schück, and the two latter gentlemen compared their instruments with the Observatory standards.

Copies of magnetic curves were supplied to Dr. J. A. Fleming, F.R.S., for use in his lecture before the British Association on "The Earth as a Great Magnet."

II. Meteorological Observations.

The several self-recording instruments for the continuous registration of Atmospheric Pressure, Temperature of Air and Wet-bulb, Wind (direction and velocity), Bright Sunshine, and Rain, have been maintained in regular operation throughout the year, and the standard eye observations for the control of the automatic records duly registered.

The tabulations of the meteorological traces have been regularly made, and these, as well as copies of the eye observations, with

notes of weather, cloud, and sunshine, have been transmitted, as usual, to the Meteorological Office.

With the sanction of the Meteorological Council, data have been supplied to the Council of the Royal Meteorological Society, the Institute of Mining Engineers, and the editor of 'Symons' Monthly Meteorological Magazine.'

During the year there occurred the death of Mr. E. Dagwell, an assistant principally occupied in the Meteorological Department; his connection with the Observatory had lasted since 1881.

Electrograph.—As a result of experimental investigations made in the present and preceding year, several important changes have been made in the water-dropping apparatus. A new water reservoir and overflow tube have been obtained, and the jet has been brought much nearer the electrometer. The auxiliary battery having considerably deteriorated, it was sent to the makers on November 20, and the instrument in consequence remained out of action until the end of the year.

Determinations of the scale value were made on March 30, June 27, July 27, and October 24, by direct comparison with the Portable Electrometer, White No. 53.

Inspections.—In compliance with the request of the Meteorological Council, the following Observatories and Anemograph Stations have been visited and inspected:—Yarmouth, North Shields, Alnwick Castle, Fort William, Glasgow, Aberdeen, and Deerness (Orkney) by Mr. Baker; and Radcliffe Observatory (Oxford), Holyhead, Fleetwood, Stonyhurst, Armagh, Dublin, Valencia, and Falmouth by Mr. Constable.

III. Solar Observations.

Sun-spots.—Sketches of Sun-spots have been made on 161 days, and the groups numbered, after Schwabe's method.

Particulars will be found in Appendix II, Table IV.

IV. Experimental Work.

Fog and Mist.—The observations of a series of distant objects, referred to in previous Reports, have been continued. A note is taken of the most distant of the selected objects which is visible at each observation hour.

Atmospheric Electricity.—The eye observations referred to in last year's Report have been discussed ('Roy. Soc. Proc.,' vol. 60, 1896, p. 96). Some further experiments have been carried out on

similar lines, and in particular comparisons have been made of the potential at the spot where the jet of the water-dropper breaks up with that at a station outside the immediate influence of the Observatory buildings.

Aneroid Barometers.—A large number of experiments have been made in continuation of those mentioned last year, and the readings have been reduced. Further experiments are contemplated. In the meantime the results are being worked up by the Superintendent.

Nocturnal Radiation.—The observations with minimum thermometers referred to last year have been continued.

Platinum Thermometers.—Observations have been made from time to time of the readings of six platinum thermometers in ice and steam, and of certain of the number in sulphur vapour and in molten silver, special attention being paid to changes of zero.

A series of comparisons of mercury and platinum thermometers at temperatures above 100° C. has been made, the thermometers being immersed in a well-stirred fusible metal bath, kindly lent by Mr. C. T. Heycock, F.R.S., and Mr. Neville. The comparisons were initiated by these gentlemen, who visited the Observatory during several days in January.

Early in the year a platinum resistance thermometer was buried underground along with two iron resistance thermometers of a type invented by Professor H. Callendar, F.R.S., and Mr. E. H. Griffiths, F.R.S. During the year readings of earth temperatures have been taken usually twice a day, and a report on the behaviour of the instruments has been submitted to the Meteorological Council, on whose behalf the experiments were undertaken.

Small inconsistencies in the behaviour of the resistance box used with the platinum thermometers having been brought to light, the Committee have arranged with Dr. Harker, late of Owens College, Manchester, to investigate the matter, so that the defect may be remedied.

Photographic Test for Lenses.—Difficulties having been repeatedly met with in applying the present tests for curvature, astigmatism, and definition, experiments have been made which may lead to replacing or supplementing these tests by direct observations and measurements on a photographic plate.

V. Verification of Instruments.

The subjoined is a list of the instruments examined in the year 1896, with the corresponding results for 1895:—

	Number tested in the year ending December 31.	
	1895.	1896.
Air-meters	5	5
Anemometers	7	12
Aneroids	254	113
Artificial horizons	15	21
Barometers, Marine	151	84
„ Standard	64	72
„ Station	25	40
Binoculars	376	455
Compasses	244	3
Deflectors	20	—
Hydrometers	187	374
Inclinometers	4	8
Photographic Lenses	14	14
Magnets	2	4
Navy Telescopes	456	546
Rain Gauges	9	17
Rain Measuring Glasses	90	26
Scales	4	1
Sextants	532	591
Sunshine Recorders	0	2
Theodolites	7	5
Thermometers, Avitreous or Immisch's	39	7
„ Clinical	16,699	13,772
„ Deep sea	125	74
„ High Range	34	52
„ Hypsometric	25	34
„ Low Range	114	62
„ Meteorological	2,647	4,098
„ Solar radiation	3	2
„ Standard	81	69
Unifilars	4	3
Vertical Force Instruments	34	0
Total	22,271	20,566

Duplicate copies of corrections have been supplied in 98 cases.

The number of instruments rejected in 1895 and 1896 on account of excessive error or for other reasons was as follows :—

	1895.	1896.
Thermometers, clinical	195	161
„ ordinary meteorological..	48	56
Sextants	83	79
Telescopes	10	30
Various	38	43

Five Standard Thermometers have been supplied during the year.

There were at the end of the year in the Observatory undergoing verification 10 Barometers, 962 Thermometers, 22 Sextants, 30 Hydrometers, 2 Air Meters, and 1 Sunshine Recorder.

VI. Rating of Watches and Chronometers.

The high standard of excellence to which attention was drawn in last year's Report has been fully maintained, and there has been a marked increase in the number of watches which have obtained the highest possible form of certificate—the class A especially good—(involving the attainment of 80 per cent. of the total marks), no less than 96 being so classed.

The 583 watches received were entered for trial as below :—

For class A, 400; class B, 115; class C, 50; and 18 for the subsidiary trial. Of these 14 passed the subsidiary test, 118 failed from various causes to gain any certificate; 25 were awarded class C certificates, 111 class B, and 315 class A; of the latter, 96 obtained the highest form of certificate, class A, *especially good.*

In Appendix III will be found a table giving the results of trial of the 96 watches which gained the highest number of marks during the year. The first place was taken by Messrs. Stauffer, Son, & Co., London, with a keyless, going-barrel, chronometer-watch, No. 147,545, with the "Tourbillon" escapement, which obtained 89·4 marks out of a maximum of 100. The performance of the first 4 watches on the list, embracing 1 "Tourbillon" movement, 2 Bar-levers, and 1 "Karrusel," is very fine, and the marks obtained are unusually close.

Owing to considerations of space, it is intended to publish in future annual Reports particulars as to the first 50 watches only which come up to the standard of 80 marks and over. If a larger number should reach that standard, they will be mentioned in a full list which will be sent for publication to the horological journals.

It is intended shortly to suspend the class C test for watches.

Marine Chronometers.—During the year, 61 chronometers have been entered for the Kew A and B trials, of which 51 were certificated, and 10 failed to pass.

The new gas boiler for the watch oven has been very successful, and the difficulty of regularly maintaining the temperature at about 90° F. has been greatly reduced, and a considerably higher temperature could be easily obtained.

The improvements to the refrigerator mentioned in last year's Report have also proved of considerable advantage.

The mean time standard clock "French" has had new seconds contact pieces, &c., fitted to it by Dent & Co., and has been fixed in a new position in the South Hall, where the daily range of temperature is but small.

VII. Miscellaneous.

Paper.—Prepared photographic paper has been supplied to the Observatories at Hong Kong, Lisbon, Mauritius, St. Petersburg, Toronto, Oxford (Radcliffe), and Stonyhurst, and through the Meteorological Office to Aberdeen, Batavia, Fort William, and Valencia.

Anemograph and Sunshine Sheets have also been sent to Hong Kong and Mauritius, and *papier Saxe* to Coimbra.

Exhibition at Glasgow.—A selection of photographic curves from the various self-recording instruments, along with photographs of clouds and of sun spots, was shown during the summer at an exhibition promoted by the Corporation of Glasgow. The exhibits have all been safely returned.

House, Grounds, and Paths.—These have been kept as usual throughout the year.

Dines' Pressure Tube Anemometer.—A self-recording tube anemometer belonging to the Meteorological Office was erected on the roof early in December. Its vane is approximately at the same level as the cups of the standard "Robinson" Anemometer.

Ships' Compasses.—The test applied to ships' compasses has been revised and extended, with the kind assistance of Captain E. W. Creak, R.N., F.R.S., so as to bring it more into line with that approved by the Admiralty; and it is intended shortly to put the new regulations into operation. With a view to doing so, an apparatus has been obtained from Mr. A. W. Dover, similar to that employed at Deptford, for determining the accuracy of the compass bowl and card and the strength of the magnets.

Travellers' Azimuth Compasses.—Special experiments, approved by Mr. Francis Galton, Chairman of the Committee, have been applied to a variety of old and new compasses, submitted on behalf of the

Kew Observatory. Account of Receipts and Payments for the year ending December 31st, 1896.

RECEIPTS.

	£	s.	d.	£	s.	d.
To Balance from Year 1895				341	7	9
Royal Society:—						
Gassiot Trust. Annual payment				444	11	4
Meteorological Council:—	£	s.	d.			
Allowance	400	0	0			
Postages, &c.	18	3	1			
				418	3	1
Tests:—						
Verifications	1378	10	8			
Rating	615	9	4			
Lenses	2	3	10			
				1996	3	10
Commissions executed for Colonial and Foreign Institutions, &c.				265	1	1
Rents				11	10	6
Dividends on India Stock				30	9	0
London and County Bank for Unused Cheques				0	4	10
				£3507	11	5

PAYMENTS.

	£	s.	d.	£	s.	d.
By Normal Observatory:—						
Salaries—Observations, Tabulations, &c.	301	17	8			
Incidental Expenses, Instruments, &c.	77	1	4			
Proportion of Administration Expenditure	237	0	0			
				615	19	0
Researches:—						
Purchase of Apparatus, &c.	153	6	1			
Proportion of Administration Expenditure	355	10	0			
				508	16	1
Tests:—						
Salaries	812	3	6			
Incidental Expenses—Instruments, Postages, &c.	189	14	11			
Proportion of Administration Expenditure	475	19	6			
				1477	17	11
Commissions:—						
Purchase of Instruments and Photographic Paper for Colonial and Foreign Institutions, &c.	185	6	3			
Proportion of Administration Expenditure	118	10	0			
				303	16	3
Purchase of £400 India 3½ per cent. Stock				471	1	0
Balance in London and County Bank	119	18	8			
Balance in hand (Petty Cash)	10	2	6			
				130	1	2
				£3507	11	5

Audited on behalf of the Royal Society and found correct.
12th of January, 1897.
(Signed) W. B. KEEN, *Chartered Accountant.*

Examined on the part of the Committee and found correct.
14th of January, 1897.
(Signed) ROBERT H. SCOTT.

ADMINISTRATION EXPENDITURE.

Particulars.	£	s.	d.
Superintendent	500	0	0
First Assistant, Librarian, &c.	379	10	1
Rent, Fuel, &c.	88	13	3
Caretaker, Repairs, &c.	218	16	2
	£1186	19	6

Apportionment.	£	s.	d.
Observatory	237	0	0
Researches	355	10	0
Tests	475	19	6
Commissions	118	10	0
	£1186	19	6

ESTIMATED ASSETS.

	£	s.	d.	£	s.	d.
By Balance as per Statement				130	1	2
£1300 India 3½ per cent. Stock, value on January 1, 1897				1542	2	6
Payments due:—						
Meteorological Council—Allowance, Postages, &c.	109	4	2			
Test Fees	600	13	4			
Commissions	34	4	0			
				744	1	6
Stock:—						
Blank Forms and Certificates	45	9	5			
Standard Thermometers	79	0	0			
				124	9	5
				£2540	14	7

January 22nd, 1897.

ESTIMATED LIABILITIES.

	£	s.	d.
To Administration accounts—Gas, Rent, Repairs, &c.	33	13	5
Observatory accounts—Photographic Paper, &c.	8	2	0
Tests accounts—Repairs, Apparatus, &c.	38	0	3
Commissions	26	19	0
General Balance	2433	19	11
	£2540	14	7

(Signed) CHARLES CHREE,
Superintendent.

Royal Geographical Society, to whom a report will shortly be submitted.

Library.—During the year the Committee lost the valuable services of Mr. R. S. Whipple, son of the late Superintendent, who had acted as Librarian and Accountant. The vacancy thus occasioned on the staff has been filled by the appointment of Mr. G. E. Bailey, previously employed in library and bursarial work at King's College, Cambridge.

The number of publications received during the year was much as usual. Volumes 14 to 36 of the 5th Series of the 'Philosophical Magazine' have been purchased to fill a gap.

Audit, &c.—The accounts for 1896 have been audited by Mr. W. B. Keen, Chartered Accountant, on behalf of the Royal Society, and by Mr. R. H. Scott on behalf of the Committee.

The present system of book-keeping having now been in operation for two years, the comparison of the expenditure of the present and previous years, suspended in 1895, has been resumed; it will be found on the opposite page.

The Committee found themselves in a position to purchase in November £400 India $3\frac{1}{2}$ per cent. Inscribed Stock.

Personal Establishment.

The staff employed is as follows:—

C. Chree, Sc.D., Superintendent.
T. W. Baker, Chief Assistant.
E. G. Constable, Observations and Rating.
W. Hugo, Verification Department.
J. Foster ,, ,,
T. Gunter ,, ,,
W. J. Boxall ,, ,,
G. E. Bailey, Accounts and Library.
E. Boxall, Observations and Rating, and six other Assistants.
A Caretaker and Housekeeper are also employed.

(Signed) FRANCIS GALTON,
Chairman.

Comparison of Expenditure during the Years 1895 and 1896.

Expenditure.	1895.			1896.			Increase.			Decrease.		
	£	*s.*	*d.*	£	*s.*	*d.*	£	*s.*	*d.*	£	*s.*	*d.*
Administration :—												
Superintendent.....	400	0	0	500	0	0	100	0	0			
First Assistant.....	275	6	0	273	18	0				1	8	0
Office.....	106	10	0	105	12	1				0	17	11
Rent, Fuel, Lighting, &c.	88	18	11	88	13	3				0	5	8
Caretaker.....	68	18	0	68	18	0						
Incidental Expenses....	140	12	5	149	18	2	9	5	9			
	1080	5	4	1186	19	6	109	5	9	2	11	7
Normal Observatory :—												
Salaries—Observations, &c.....	351	19	6	301	17	8				50	1	10
Incidental Expenses	56	9	3	77	1	4	20	12	1			
Prop. Adm. Expenditure	108	0	0	237	0	0	129	0	0			
Researches :—												
Purchase of Apparatus..	75	19	1	153	6	1	77	7	0			
Platinum Thermometer Room	123	14	6							123	14	6
Prop. Adm. Expenditure	216	0	0	355	10	0	139	10	0			
Tests :—												
Salaries.....	797	4	6	812	3	6	14	19	0			
Incidental Expenses....	167	15	3	189	14	11	21	19	8			
Prop. Adm. Expenditure	648	5	4	475	19	6				172	5	10
Commissions :—												
Purchases for Colonial Institutions, &c.	269	11	3	185	6	3				84	5	0
Prop. Adm. Expenditure	108	0	0	118	10	0	10	10	0			
Construction of Fence and Roadway	74	0	0							74	0	0
Purchase of Stock	1039	4	6	471	1	0				568	3	6
Gross Expenditure.... (showing a decrease of £658 12*s.* 11*d.*).	4036	3	2	3377	10	3	413	17	9	1072	10	8
Extraordinary Expenditure.												
Researches :—												
Purchase of Apparatus, &c.	70	2	2	150	4	2	80	2	0			
Platinum Thermometer Room	123	14	6							123	14	6
Commissions :—												
Purchases for Colonial Institutions, &c.....	269	11	3	185	6	3				84	5	0
Construction of Fence and Roadway.....	74	0	0							74	0	0
Purchase of Stock	1039	4	6	471	1	0				568	3	6
	1576	12	5	806	11	5	80	2	0	850	3	0
Leaving for Ordinary Nett Expenditure..... (showing an increase of £111 8*s.* 1*d.*).	2459	10	9	2570	18	10	333	15	9	222	7	8

List of Instruments, Apparatus, &c., the Property of the Kew Observatory Committee, at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	Date of loan.
G. J. Symons, F.R.S.	Portable Transit Instrument	1869
The Science and Art Department, South Kensington.	Articles specified in the list in the Annual Report for 1893.....	1876
Professor W. Grylls Adams, F.R.S.	Unifilar Magnetometer, by Jones, No. 101, complete.....	1883
	Pair 9-inch Dip-Needles with Bar Magnets ...	1887
Lord Rayleigh, F.R.S.	Standard Barometer (Adie, No. 655)	1885
The "Jackson-Harmsworth" Polar Expedition.	Unifilar Magnetometer, by Jones, marked N.A.B.C., complete. Dip-Circle, by Barrow, with two Needles and Bar Magnets. Two Tripod Stands	1894

APPENDIX I.

Magnetical Observations, 1896.

Made at the Kew Observatory, Old Deer Park, Richmond, Lat. 51° 28′ 6″ N. and Long. $0^h\ 1^m\ 15^s{\cdot}1$ W.

The results given in the following tables are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet K.C. I. and the Declinometer Magnet marked K.O. 90 in the 9-inch Unifilar Magnetometer by Jones.

The Inclination was observed with the Inclinometer by Barrow, No. 33, and needles 1 and 2, which are $3\frac{1}{2}$ inches in length.

The Declination and Force values given in Tables I to VIII are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing Magnetic Observations.

The following is a list of the days during the year 1896 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal inequalities, and which have been employed in the preparation of the magnetic tables :—

January	1, 2, 21, 24, 29.
February	7, 18, 20, 23, 24.
March	11, 16, 17, 18, 21.
April.....	7, 14, 16, 20, 30.
May	5, 6, 9, 26, 29.
June.....	2, 7, 20, 23, 24.
July	2, 9, 17, 19, 31.
August.....	5, 13, 16, 27, 28.
September	8, 9, 10, 25, 28.
October.....	6, 7, 18, 25, 26.
November	3, 12, 22, 24, 25.
December.....	8, 12, 18, 19, 24.

Table I.—Hourly Means of Declination, as determined from the

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
	(17° +) West						Winter.						
1896. Months.	′	′	′	′	′	′	′	′	′	′	′	′	′
Jan. ..	14·7	11·8	12·4	12·8	12·9	13·0	13·1	12·9	12·8	12·3	11·9	12·7	14·7
Feb. ..	17·0	12·1	12·6	12·8	12·6	12·5	12·4	12·4	12·2	11·9	12·3	13·2	14·9
March.	18·4	12·2	12·2	12·4	12·1	12·1	11·9	11·8	11·3	9·9	9·6	11 ·8	14·8
Oct. ..	14·3	9·0	8·8	9·1	9·0	9·0	8·7	8·4	7·9	7·2	7·6	9·6	12·2
Nov. ..	12·6	8·5	8·7	8·9	9·1	9·2	9·2	8·7	8·5	8·2	7·9	9·1	10·8
Dec. ..	10·7	7·4	7·8	8·1	8·3	8·5	8·5	8·3	8·2	8·2	8·3	8·3	9·3
Mean	14·6	10·2	10·4	10·7	10·7	10·7	10·6	10·4	10·2	9·6	9·6	10·8	12·8
						Summer.							
	′	′	′	′	′	′	′	′	′	′	′	′	′
April..	15·4	9·6	10·0	10·2	10·3	9·9	9·8	8·6	7·1	6·3	6·2	8·2	11·4
May ..	14·5	9·9	9·8	9·6	9·2	8·4	7·3	6·2	5·4	5·6	6·5	9·0	11·4
June ..	14·9	8·8	8·8	8·3	8·2	7·7	6·4	5·3	5·2	5·7	7·1	9·1	11·6
July ..	14·2	9·6	9·6	9·4	8·8	8·4	7·4	6·3	5·8	6·3	7·6	9·5	11·8
Aug. ..	15·3	8·5	8·8	8·6	8·2	7·9	7·3	6·7	6·3	6·5	7·8	10·2	12·9
Sept. ..	16·0	8·7	8·8	8·7	8·5	8·2	7·7	6·9	6·0	5·3	6·4	9·8	13·2
Mean	15·1	9·2	9·3	9·1	8·9	8·4	7·6	6·7	6·0	6·0	6·9	9·3	12·0

Table II.—Diurnal Inequality of the Kew

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
					Summer Mean.							
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·1	−0·9	−1·1	−1·4	−1·8	−2·6	−3·6	−4·3	−4·3	−3·3	−0·9	+1·8
					Winter Mean.							
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·2	−1·0	−0·7	−0·7	−0·7	−0·8	−1·0	−1·2	−1·8	−1·8	−0·6	+1·4
					Annual Mean.							
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·1	−1·0	−0·9	−1·0	−1·3	−1·7	−2·3	−2·7	−3·0	−2·6	−0·8	+1·6

NOTE.—When the sign is + the magnet

selected quiet Days in 1896. (The Mean for the Year = 17° 10′·8 West.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon.
					Winter.								
′	′	′	′	′	′	′	′	′	′	′	′	′	′
16·0	16·9	16·4	15·2	14·4	13·7	13·3	13·0	12·8	12·4	12·1	11·3	11·0	16·0
16·1	17·0	17·1	16·7	15·2	14·6	13·7	13·3	13·1	12·4	12·4	11·7	11·5	16·7
17·7	19·1	18·8	17·4	15·5	14·0	13·6	13·2	12·8	12·4	12·3	12·3	12·2	18·1
14·2	14·7	13·8	12·4	11·2	10·6	10·4	10·0	9·7	9·5	9·3	9·2	8·9	14·5
12·0	12·2	11·5	10·6	10·1	9·6	9·3	9·1	8·9	8·8	8·3	8·2	8·4	12·2
9·9	10·5	10·0	9·7	9·0	8·8	8·3	8·3	8·1	8·0	7·7	8·0	7·9	11·2
14·3	15·1	14·6	13·7	12·6	11·9	11·4	11·1	10·9	10·6	10·3	10·1	10·0	14·8
					Summer.								
′	′	′	′	′	′	′	′	′	′	′	′	′	′
15·1	17·1	17·0	15·8	14·4	12·8	11·7	11·4	11·0	11·0	10·7	10·5	10·2	16·2
13·8	15·2	15·6	14·8	13·1	12·1	11·0	10·8	10·5	10·2	10·1	9·7	9·6	14·4
14·3	15·4	15·3	14·2	12·9	12·1	11·6	11·4	11·1	10·5	10·4	10·3	9·6	14·1
14·2	15·9	15·9	14·8	12·9	11·6	11·2	10·8	10·6	10·4	10·6	10·1	9·7	14·6
15·1	16·1	15·4	13·7	11·8	10·7	9·8	9·6	9·3	9·3	9·0	8·7	8·7	14·5
15·8	16·5	15·5	13·5	11·1	10·0	9·4	9·2	8·7	8·6	8·3	8·4	8·3	16·3
14·7	16·0	15·8	14·5	12·7	11·5	10·8	10·5	10·2	10·0	9·9	9·6	9·4	15·0

Declination as derived from Table I.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
					Summer Mean.							
′	′	′	′	′	′	′	′	′	′	′	′	′
+4·5	+5·8	+5·5	+4·2	+2·5	+1·3	+0·5	+0·3	0·0	−0·2	−0·4	−0·6	−0·9
					Winter Mean.							
′	′	′	′	′	′	′	′	′	′	′	′	′
+2·9	+3·7	+3·2	+2·3	+1·2	+0·5	+0·1	−0·2	−0·5	−0·8	−1·0	−1·3	−1·4
					Annual Mean.							
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·7	+4·7	+4·4	+3·3	+1·8	+0·9	+0·3	0·0	−0·3	−0·5	−0·7	−1·0	−1·1

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force in C.G.S. units (corrected
(The Mean for the

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
	0·18000 +					Winter.							
1896. Months.													
Jan. ...	284	297	300	299	300	304	304	306	307	308	300	294	292
Feb. ...	280	299	297	296	296	299	302	304	302	301	294	288	282
March ..	279	299	300	301	301	300	301	301	299	292	282	278	275
Oct. ...	304	326	325	324	323	325	325	324	321	315	307	301	300
Nov. ...	308	320	319	319	319	320	322	324	322	319	314	309	307
Dec. ...	310	318	318	319	319	320	322	323	324	324	322	318	317
Mean..	294	310	310	310	310	311	313	314	312	310	303	298	295
						Summer.							
April...	281	306	306	306	309	310	308	311	308	301	290	283	280
May ...	278	304	303	303	302	302	302	300	296	290	287	285	284
June ...	292	310	307	306	305	306	306	301	294	290	287	287	295
July ...	300	323	322	322	322	322	321	317	313	307	303	300	303
Aug. ...	299	317	316	315	313	311	311	309	305	294	287	283	287
Sept. ...	295	312	313	313	312	311	310	307	299	288	278	275	281
Mean..	291	312	311	311	311	310	310	308	303	295	289	286	288

Table IV.—Diurnal Inequality of the Kew

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
					Summer Mean.							
	+ ·00003	+ ·00002	+ ·00002	+ ·00002	+ ·00001	+ ·00001	− ·00001	− ·00006	− ·00014	− ·00020	− ·00023	− ·00021
					Winter Mean.							
	+ ·00001	+ ·00001	+ ·00001	+ ·00001	+ ·00002	+ ·00004	+ ·00005	+ ·00003	+ ·00001	− ·00006	− ·00011	− ·00014
					Annual Mean.							
	+ ·00002	+ ·00002	+ ·00001	+ ·00001	+ ·00002	+ ·00002	+ ·00002	− ·00002	− ·00007	− ·00013	− ·00017	− ·00017

NOTE.—When the sign is + the

for Temperature) as determined from the selected quiet Days in 1896. Year = 0·18309.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon.
Winter.													
292	295	298	301	302	303	305	307	305	304	304	300	299	281
284	289	294	294	294	293	294	299	302	304	304	305	304	287
281	290	298	301	302	302	305	306	306	306	308	305	305	283
305	309	315	317	317	320	324	329	329	329	327	326	326	311
311	317	319	321	322	325	325	324	323	323	323	321	320	317
318	320	319	319	320	322	322	322	323	322	321	322	320	315
298	303	307	309	309	311	312	314	315	315	314	313	312	299
Summer.													
284	294	304	311	314	318	318	318	319	318	317	317	316	284
286	289	296	303	309	313	317	318	315	314	315	313	311	288
297	301	310	313	315	317	319	321	320	319	317	315	314	294
309	312	318	325	327	330	332	332	333	331	329	329	326	309
298	309	314	318	318	318	321	321	322	321	319	319	318	299
294	306	312	314	314	315	316	320	321	320	319	318	317	290
295	302	309	314	316	319	321	322	322	321	319	319	317	294

Horizontal Force as deduced from Table III.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Mean.												
−·00014	−·00007	·00000	+·00005	+·00007	+·00010	+·00012	+·00013	+·00013	+·00012	+·00010	+·00010	+·00008
Winter Mean.												
−·00011	−·00006	−·00002	·00000	·00000	+·00002	+·00003	+·00005	+·00006	+·00006	+·00005	+·00004	+·00003
Annual Mean.												
−·00012	−·00007	−·00001	+·00002	+·00004	+·00006	+·00008	+·00009	+·00009	+·00009	+·00008	+·00007	+·00006

reading is above the mean.

Table V.—Hourly Means of the Kew Vertical Force in C.G.S. units (corrected
(The Mean for the

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
	0·43000 +					Winter.							
1896. Months.													
Jan. ...	888	889	889	889	889	889	889	889	889	888	888	888	888
Feb. ...	875	884	883	882	883	884	884	884	884	885	885	883	882
March ..	902	914	914	913	912	912	911	911	911	911	907	901	897
Oct. ...	924	932	931	932	933	933	933	933	933	933	929	924	922
Nov. ...	944	943	943	943	944	944	943	943	942	943	942	938	937
Dec. ...	900	907	908	907	908	909	909	909	909	908	908	907	908
Mean	905	911	911	911	911	912	911	911	911	911	909	907	906
						Summer.							
April ...	916	932	932	932	932	931	933	934	934	932	927	919	912
May ...	926	941	941	941	941	942	942	942	941	936	930	922	914
June ...	945	962	962	962	963	965	967	964	962	960	955	948	943
July ...	955	973	973	973	974	976	978	977	976	974	969	966	963
Aug. ...	908	925	924	925	926	929	930	932	931	929	924	917	912
Sept. ...	897	907	907	907	907	907	908	910	910	909	904	896	893
Mean	924	940	940	940	941	942	943	943	942	940	935	928	923

Table VI.—Diurnal Inequality of the Kew

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
					Summer Mean.							
	+ ·00003	+ ·00003	+ ·00003	+ ·00004	+ ·00005	+ ·00006	+ ·00006	+ ·00005	+ ·00003	− ·00002	− ·00009	− ·00014
					Winter Mean.							
	·00000	·00000	·00000	·00000	+ ·00001	·00000	·00000	·00000	·00000	− ·00002	− ·00004	− ·00006
					Annual Mean.							
	+ ·00001	+ ·00002	+ ·00001	+ ·00002	+ ·00003	+ ·00003	+ ·00003	+ ·00003	+ ·00002	− ·00002	− ·00007	− ·00010

Note.—When the sign is + the

for Temperature), as determined from the selected quiet Days in 1896. Year = 0·43924.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon.
Winter.													
890	891	894	894	894	893	892	891	891	891	891	890	891	888
881	883	884	887	891	891	890	889	888	888	888	887	886	883
897	900	904	909	911	911	910	909	908	907	906	906	906	889
924	927	931	937	938	937	936	935	934	934	934	934	934	926
937	940	942	945	945	944	943	943	942	942	941	941	941	945
908	909	911	912	915	914	914	913	912	912	911	911	911	907
906	908	911	914	916	915	914	913	912	912	912	911	911	906
Summer.													
909	913	920	924	927	929	930	930	929	927	926	925	925	908
913	916	920	925	928	929	930	929	929	928	928	928	927	904
943	946	952	957	962	963	963	963	963	962	961	959	959	936
962	962	968	973	976	978	978	977	976	974	973	972	973	954
914	921	928	931	932	933	933	932	931	931	930	930	930	912
893	897	902	907	911	912	911	911	910	911	909	910	909	893
922	926	932	936	939	941	941	940	940	939	938	937	937	918

Vertical Force as deduced from Table V.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10	11.	Mid.
Summer Mean.												
−·00015	−·00011	−·00005	−·00001	+·00002	+·00004	+·00004	+·00003	+·00003	+·00002	+·00001	·00000	·00000
Winter Mean.												
−·00005	−·00003	·00000	+·00003	+·00004	+·00004	+·00003	+·00002	+·00001	+·00001	+·00001	·00000	·00000
Annual Mean.												
−·00010	−·00007	−·00003	+·00001	+·00003	+·00004	+·00003	+·00003	+·00002	+·00002	+·00001	·00000	·00000

reading is above the mean.

Table VII.—Hourly Means of the Inclination, calculated from the Horizontal

Hours	Preceding noon.	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
					67° +			Winter.					
1896. Months.	′	′	′	′	′	′	′	′	′	′	′	′	′
Jan.....	23·0	22·1	21·9	22·0	21·9	21·7	21·7	21·5	21·5	21·4	21·9	22·3	22·4
Feb.....	22·9	21·9	22·0	22·0	22·0	21·9	21·7	21·5	21·7	21·8	22·2	22·6	23·0
March..	23·7	22·7	22·6	22·5	22·5	22·6	22·5	22·5	22·6	23·1	23·6	23·7	23·8
Oct.....	22·7	21·4	21·4	21·5	21·6	21·5	21·5	21·6	21·8	22·2	22·6	22·9	22·9
Nov.	22·9	22·1	22·2	22·2	22·2	22·1	22·0	21·8	21·9	22·2	22·5	22·7	22·8
Dec.....	21·6	21·2	21·3	21·2	21·2	21·2	21·0	21·0	20·9	20·9	21·0	21·2	21·3
Mean..	22·8	21·9	21·9	21·9	21·9	21·8	21·7	21·6	21·7	21·9	22·3	22·6	22·7
						Summer.							
	′	′	′	′	′	′	′	′	′	′	′	′	′
April...	24·0	22·7	22·7	22·7	22 5	22·4	22·6	22·5	22·7	23·1	23·7	23·9	23·9
May....	24·4	23·1	23·2	23·2	23 2	23·3	23·3	23·4	23·7	23·9	24·0	23·9	23·7
June ...	24·0	23·3	23·5	23·6	23·7	23·7	23·7	24·0	24·4	24·6	24·6	24·5	23·8
July....	23·8	22·8	22·8	22·8	22·8	22·9	23·0	23·3	23·5	23·8	24·0	24·1	23·8
Aug. ...	22·5	21·8	21·9	22·0	22·1	22·3	22·3	22·5	22·8	23·4	23·8	23·9	23·4
Sept. ...	22·5	21·6	21·6	21·6	21·6	21·7	21·8	22·1	22·6	23·3	23·8	23·8	23·3
Mean..	23·5	22·6	22·6	22·7	22·7	22·7	22·8	23·0	23·3	23·7	24·0	24·0	23·7

Table VIII.—Diurnal Inequality of the

Hours	Mid.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	
					Summer Mean.							
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·1	−0·1	0·0	0·0	0·0	+0·1	+0·3	+0·6	+1·0	+1·3	+1·3	+1·0
					Winter Mean.							
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·1	−0·1	−0·1	−0·1	−0·1	−0·2	−0·3	−0·2	0·0	+0·4	+0·6	+0·8
					Annual Mean.							
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·1	−0·1	0·0	0·0	0·0	−0·1	0·0	+0·2	+0·5	+0·8	+1·0	+0·9

NOTE.—When the sign is +

and Vertical Forces (Tables III and V). (The Mean for the Year = 67° 22′·3.)

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.	Succeeding noon.
Winter.													
′	′	′	′	′	′	′	′	′	′	′	′	′	′
22·5	22·3	22·2	22·0	22·0	21·9	21·7	21·5	21·7	21·7	21·7	22·0	22·1	23·2
22·8	22·5	22·2	22·3	22·4	22·5	22·4	22·0	21·8	21·6	21·6	21·6	21·6	22·6
23·4	22·9	22·5	22·4	22·4	22·4	22·2	22·1	22·1	22·0	21·9	22·1	22·1	23·1
22·6	22·4	22·1	22·1	22·2	21·9	21·6	21·3	21·2	21·2	21·4	21·4	21·4	22·2
22·5	22·2	22·1	22·1	22·0	21·8	21·8	21·8	21·9	21·9	21·8	22·0	22·0	22·4
21·3	21·2	21·3	21·3	21·3	21·2	21·2	21·1	21·1	21·1	21·2	21·1	21·2	21·4
22·5	22·2	22·1	22·0	22·0	21·9	21·8	21·6	21·6	21·6	21·6	21·7	21·7	22·5
Summer.													
′	′	′	′	′	′	′	′	′	′	′	′	′	′
23·6	23·0	22·5	22·2	22·1	21·9	21·9	21·9	21·8	21·8	21·9	21·8	21·9	23·6
23·5	23·4	23·1	22·8	22·4	22·2	21·9	21·9	22·1	22·1	22·0	22·2	22·3	23·2
23·7	23·5	23·0	23·0	23·0	22·9	22·7	22·6	22·7	22·7	22·8	22·9	23·0	23·6
23·4	23·2	22·9	22·6	22·6	22·3	22·2	22·2	22·1	22·1	22·3	22·3	22·6	23·2
22·8	22·2	22·1	21·9	21·9	22·0	21·8	21·7	21·6	21·7	21·8	21·8	21·9	22·6
22·5	21·8	21·5	21·5	21·6	21·6	21·5	21·2	21·1	21·2	21·2	21·3	21·4	22·7
23·3	22·9	22·5	22·3	22·3	22·2	22·0	21·9	21·9	21·9	22·0	22·1	22·2	23·2

Inclination as deduced from Table VII.

Noon	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·6	+0·2	−0·2	−0·4	−0·4	−0·5	−0·7	−0·8	−0·8	−0·8	−0·7	−0·6	−0·5
Winter Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·6	+0·3	+0·1	+0·1	+0·1	0·0	−0·1	−0·3	−0·3	−0·4	−0·4	−0·3	−0·2
Annual Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+0·6	+0·2	0·0	−0·1	−0·2	−0·3	−0·4	−0·5	−0·6	−0·6	−0·5	−0·4	−0·4

the reading is above the mean.

APPENDIX Ia.

MEAN VALUES, for the years specified, of the Magnetic Elements at Observatories whose Publications are received at Kew Observatory.

Place.	Latitude.	Longitude.	Year.	Declination.	Inclination.	Horizontal Force. C. G. S. Units.	Vertical Force. C. G. S. Units.
Pawlowsk	59 41 N.	30 29 E.	1894	0 10·5 E.	70 43·6 N.	·16456	·47061
Katharinenburg	56 49 N.	60 38 E.	1894	9 39·4 E.	70 40·0 N.	·17799	·50729
Kasan	55 47 N.	49 8 E.	1892	7 30·8 E.	68 36·2 N.	·18551	·47345
Copenhagen ...	55 41 N.	12 34 E.	1894	10 41·3 W.	—	·17373	—
Stonyhurst	53 51 N.	2 28 W.	1895	18 37·8 W.	68 59·2 N.	·17148	·44637
Hamburg.....	53 34 N.	10 3 E.	1895	11 42·7 W.	67 44·3 N.	·18009	·43994
Wilhelmshaven	53 32 N.	8 9 E.	1895	12 52·5 W.	67 54·5 N.	·17983	·44305
Potsdam	52 23 N.	13 4 E.	1895	10 19·9 W.	66 39·8 N.	·18720	·43392
Irkutsk.....	52 16 N.	104 16 E.	1894	2 8·0 E.	70 10·5 N.	·20116	·55796
Utrecht*.....	52 5 N.	5 11 E.	1894	14 21·1 W.	67 10·1 N.	·18416	·43737
Kew.....	51 28 N.	0 19 W.	1896	17 10·8 W.	67 22·3 N.	·18309	·43924
Greenwich†....	51 28 N.	0 0	1895	16 57·4 W.	67 15·9 N. 67 14·9 N.	·18323	·43727 ·43692
Uccle (Brussels)	50 48 N.	4 20 E.	1893	14 48·7 W.	66 28·4 N.	·1877	·4311
Falmouth	50 9 N.	5 5 W.	1895	18 54·5 W.	67 0·4 N.	·18547	·43708
Prague	50 5 N.	14 25 E.	1895	9 31·5 W.	—	·19834	—
Parc St. Maur (Paris)	48 49 N.	2 29 E.	1894	15 15·2 W.	65 5·2 N.	·19631	·42264
Vienna.....	48 15 N.	16 21 E.	1894	8 43·6 W.	63 12·1 N.	·20740	·41061
O'Gyalla (near Buda Pesth)..	—	—	1894	7 58·2 W.	—	·21054	—
Pola (on Adriatic).....	44 52 N.	13 51 E.	1895	9 47·0 W.	60 34·0 N.	·22026	·39038
Nice.....	43 43 N.	7 16 E.	1893	12 32·7 W.	60 26·4 N.	·22198	·39139
Toronto.....	43 40 N.	79 30 W.	1895	4 45·3 W.	74 34·3 N.	·16645	·60313
Perpignan	42 42 N.	2 53 E.	1894	14 5·7 W.	60 10·4 N.	·22326	·38943
Rome...	41 54 N.	12 27 E.	1891	10 45·1 W.	58 4·6 N.	·2324	·3730
Tiflis	41 43 N.	44 48 E.	1894	1 43·1 E.	55 46·9 N.	·25680	·37761
Madrid	40 25 N.	3 40 W.	1895	16 6·6 W.	—	—	—
Coimbra.....	40 12 N.	8 25 W.	1895	17 42·0 W.	59 43·6 N.	·22581	·38685
Washington‡ ..	38 55 N.	77 4 W.	1894	3 39·9 W.	70 34·3 N.	·19979	·56646
Lisbon§.....	38 43 N.	9 9 W.	1895	17 39·1 W.	58 15·7 N.	·23344	·37731
Zi-ka-wei	31 12 N.	121 26 E.	1894	2 16·5 W.	46 0·7 N.	·32613	·33785
Hong Kong....	22 18 N.	114 10 E.	1895	0 27·8 E.	31 46·6 N.	·36480	·22589
Colaba	18 54 N.	72 49 E.	1895	0 36·9 E.	20 48·5 N.	·37444	·14230
Manila.....	14 35 N.	127 11 E.	1895	0 51·6 E.	16 49·0 N.	·37808	·11426
Batavia	6 11 S.	106 49 E.	1894	1 27·6 E.	29 13·7 S.	·36749	·20563
Mauritius	20 6 S.	57 33 E.	1894	9 59·4 W.	54 41·6 S.	·23958	·33829
Melbourne.....	37 50 S.	144 58 E.	1894	8 13·6 E.	67 16·9 S.	·23426	·55956

* The Inclination and Vertical Force are from eye observations at 2 P.M.

† Of the two values of the Inclination and Vertical Force, the first is based on observations with 3-inch dip needles only, the second on combined observations with needles of 3, 6, and 9 inches.

‡ At new observatory, situated some little distance from old station.

§ New dip needles.

APPENDIX II.—Table I.

Mean Monthly Results of Temperature and Pressure. Kew Observatory. 1896.

Months.	Thermometer.								Barometer.*					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max. and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1896.						d. h.		d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Jan.....	40°9	44°6	36°6	40°6	52°3	15 2&3 P.M.	27°2	20 8 A.M.	30·362	30·936†	30 10 A.M.	29·339	14 8 A.M.	·216
Feb. ...	40·4	45·4	35·1	40·3	55·5	12 3 ,,	22·8	26 2 ,,	30·342	30·743	3 10 ,,	29·683	20 3 ,,	·209
March..	46·2	52·4	40·2	46·3	67·4	24 3 ,,	32·6	15 7 ,,	29·825	30·353	10 10 ,,	28·844	4 11 ,,	·248
April...	48·8	56·4	41·1	48·8	65·0	27 3 ,,	33·1	24 5 ,,	30·168	30·529	21 8 ,,	29·700	29 3 P.M.	·251
May ...	53·9	63·2	44·8	54·0	75·5	12 6 ,,	35·1	2 2 ,,	30·235	30·502	25 7 ,,	29·887	20 10 A.M. & NOON.	·272
June...	62·9	72·3	53·1	62·7	82·5	15 3 ,,	38·0	1 4 ,,	29·947	30·282	19 10 P.M.	29·481	9 6 A.M.	·391
July	64·3	74·4	54·2	64·3	84·3	14 3 ,,	45·3	29 4 ,,	30·022	30·308	11 11 ,,	29·709	26 2 P.M.	·389
Aug....	59·2	67·2	51·8	59·5	73·7	12 5 ,,	45·6	27 1 ,,	30·032	30·303	11 8 A.M.	29·625	26 1 A.M.	·368
Sept....	56·8	62·7	51·6	57·2	68·2	8 1 ,, / 9 2 ,,	40·0	21 6 ,,	29·773	30·516	30 MIDT.	28·739	25 10 ,,	·386
Oct.....	46·2	52·2	40·2	46·2	61·7	3 4 ,, / 8 1 ,,	27·1	28 7&8 ,,	29·741	30·516	1 0·5 A.M.	29·191	19 2 P.M.	·261
Nov. ...	40·4	45·1	35·2	40·2	49·8	12 2 ,,	25·6	7 4 ,,	30·139	30·649	24 10 ,,	29·217	14 10 ,,	·205
Dec. ...	40·1	43·7	35·4	39·6	51·8	27 2 A.M.	26·2	24 8 ,,	29·789	30·453	27 5 P.M.	28·485	5 1 A.M.	·216
Yearly Means	50·0	56·6	43·3	50·0	..	....	..	....	30·031	..	....	..	....	·284

* Reduced to 32° at M.S.L. † On January 9th the value was 30·931 inches.

This Table is compiled from "Hourly Means," vol. 1896, of the Meteorological Office.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall.*			Weather. Number of days on which were registered							Wind.† Number of days on which it was								
		Total.	Maximum.	Date.	Rain. ‡	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.
1896.		ins.	ins.																	
January.....	8·0	0·600	0·155	25	9	1	..	..	2	20	1	7	1	4	2	4	5	5	3	10
February....	6·9	0·280	0·115	20	7	..	..	..	6	14	..	2	2	10	1	3	4	6	1	6
March.....	7·4	2·870	0·625	17	21	..	2	..	2	16	2	4	1	1	2	2	9	8	4	3
April.....	6·8	0·585	0·160	10	10	..	1	..	3	11	..	5	4	2	..	..	4	6	9	7
May.....	4·8	0·175	0·155	21	3	..	..	1	10	6	..	10	11	4		..	2	2	2	3
June.....	6·4	1·640	0·435	10	15	..	..	2	3	9	..	5	1	4	3	5	7	3	2	3
July.....	5·9	1·280	0·430	26	8	..	..	2	6	10	..	5	2	1	2	6	7	5	3	7
August.....	7·0	1·875	0·400	31	15	..	..	1	1	13	..	9	5	1	..	1	7	4	4	6
September...	7·3	5·065	0·870	12	21	..	..	..	3	16	..	2	2	1	2	5	13	3	2	3
October.....	6·6	2·380	0·540	6	18	..	1	1	7	15	1	7	3	..	..	3	11	4	3	5
November...	6·4	1·085	0·470	7	8	..	..	..	8	15	2	8	7	3	1	1	4	4	2	7
December...	8·4	3·145	0·550	2	20	1	..	..	2	21	..	4	1	2	2	8	7	5	2	4
Totals and means.	6·8	20·980			155	2	4	7	53	166	6	68	40	33	15	38	80	55	37	64

* Measured at 10 A.M. daily by gauge 1·75 feet above ground. † As registered by the anemograph.

‡ The number of rainy days are those on which 0·01 inch rain or melted snow was recorded.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.				Maximum temperature in sun's rays. (Black bulb *in vacuo*.)			Minimum temperature on the ground.			Horizontal movement of the air.*		
	Total number of hours recorded.	Mean percentage of possible sunshine.	Greatest daily record.	Date.	Mean.	Highest.	Date. †	Mean.	Lowest.	Date. ‡	Average hourly velocity.	Greatest hourly velocity.	Date.
1896.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
January	29 12	11	5 42	29	63	92	17	32	16	20	8·7	35	15
February	59 48	21	7 48	12	74	99	12	28	9	26	8·6	28	8
March	87 6	23	9 6	31	97	116	24	32	18	30	12·8	40	3
April	144 18	34	11 18	28	109	124	26	32	20	2	8·5	28	11, 12
May	233 24	48	13 48	10	118	130	19	37	20	2	10·1	30	9
June	228 0	46	14 18	1	130	139	28	46	26	1	8·5	26	9
July	225 48	45	14 18	5, 6	131	146	7	46	35	23, 28	8·4	25	25
August	141 24	31	11 18	20	120	134	23	44	34	27	7·2	21	9
September	94 24	25	10 0	19	108	126	5	46	30	21	10·0	31	23, 25, 27
October	96 36	30	8 36	5	94	116	2	34	20	28	10·3	35	6
November	67 48	26	6 36	19	69	91	3	29	17	30	10·2	36	28
December	28 42	11	5 42	25	61	85	3	28	18	19	11·5	34	14
Totals and Means	1436 30	29	..	..	98	..	..	36	..	..	9·6	..	..

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.
† Read at 10 A.M., and entered to previous day. ‡ Read at 10 A.M., and entered to same day.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days apparently without spots.
1896.			
January	11	8	—
February.....	15	14	—
March.....	16	7	1
April.....	17	8	3
May.....	15	8	—
June	17	9	—
July.....	15	8	—
August	9	5	1
September.....	12	7	—
October.....	14	6	1
November.....	13	9	1
December	7	10	—
Totals for 1896	161	99	7

APPENDIX III.—Table I.

Results of Watch Trials. Performance of the 96 Watches which obtained the highest number of marks during the year.

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate.					Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference between extreme gaining and losing rates.	Marks awarded for			Total Marks.
			Pendant up.	Pendant right.	Pendant left.	Dial up.	Dial down.				Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	
											0—40	0—40	0—20	0—100.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
Stauffer, Son, & Co., London.....	147545	Single overcoil, g.b., "tourbillon" chronometer	−1·9	−1·8	−2·4	−1·9	−2·6	0·25	0·06	4·0	34·5	38·8	16·1	89·4
U. Montandon-Robert, Geneva ...	1058	Single overcoil, d.r., g.b.	+2·4	+1·8	+2·0	+1·9	+3·2	0·4	0·03	3·5	32·9	38·2	18·2	89·3
A. E. Fridlander, Coventry	25510	Single overcoil, s.r., g.b., "Karrusel".....	−4·5	−4·3	−4·7	−4·1	−4·3	0·4	0·02	2·0	31·4	39·2	18·5	89·1
U. Montandon-Robert, Geneva...	1053	Single overcoil, d.r., g.b.	−1·1	−1·0	−1·7	−1·2	−1·8	0·4	0·04	4·0	32·7	38·8	17·6	89·0
A. E. Fridlander, Coventry	25513	Single overcoil, s.r., g.b., "Karrusel".....	+4·0	+3·4	+4·7	+5·1	+6·3	0·4	0·04	4·2	33·0	36·9	17·4	87·3
U. Montandon-Robert, Geneva...	1059	Single overcoil, d.r., g.b.	−0·5	−2·5	−0·7	−0·9	−0·5	0·3	0·06	3·5	33·8	37·5	15·9	87 2
Baume & Co., London..... ...	103037	Single overcoil, g.b., "tourbillon" chronometer.....	+1·0	+0·6	+0·5	+1·8	+3·3	0·25	0·07	5·5	35·4	36·4	15·3	87·1
U. Montandon-Robert, Geneva...	1061	Single overcoil, d.r., g.b.	+2·7	+1·8	+0·9	+0·8	+3·0	0·4	0·03	5·5	31·9	36·7	17·9	86·5
J. Adams, Coventry.....	463560	Single overcoil, s.r., g b., "Karrusel".....	+2·2	+1·4	+1·1	+3·3	+2·8	0·4	0·03	4·0	31·5	37·1	17·9	86·5
T. Russell & Son, Liverpool..... ..	89598	Single overcoil, s.r., g.b., "Karrusel"	−2·0	−1·6	−2·2	−1·1	−4·4	0·4	0·05	5·2	32·7	36·6	16·7	86·0
S. Yeomans, Coventry	73931	Single overcoil, s.r., g.b., "Karrusel"	−1·7	−1·6	−1·6	−0·3	−0·3	0·5	0·03	3·7	30·4	37·4	18·2	86·0
Wright and Craighead, London	8104	Single overcoil, s.r., g b., "Karrusel".....	−3·2	−3·4	−2·5	−1·2	+0·1	0·4	0·02	5·0	31·8	35·2	18·6	85·6
P. Ditisheim, Chaux-de-Fonds ...	1190	Single overcoil, s.r., g.b., "Karrusel".....	+1·5	+1·0	+1·9	+0·6	+2·7	0·5	0·02	5·0	29·2	37·6	18·8	85·6
Baume & Co., London.....	103031	Single overcoil, g.b., "tourbillon" chronometer.....	+2·8	+3·5	+3·3	+2·6	+4·7	0·25	0·12	7·0	35·3	37·8	12·3	85·4
U. Montandon-Robert, Geneva...	1055	Single overcoil, d.r., g.b.	−2·9	−2·7	−2·5	−2·3	−0·3	0·4	0·06	4·5	32·0	37·1	16·3	85·4
A. E. Fridlander, Coventry	52909	Single overcoil, d.r., g.b.	−1·2	−3·7	−2·0	+0·7	−1·7	0·4	0·04	5·7	32·4	35·7	17·1	85·2
U. Montandon-Robert, Geneva ..	1056	Single overcoil, d.r., g.b.	+2·1	+2·8	+3·7	+4·5	+4·1	0·5	0·02	4·7	29·9	36·6	18·7	85·2
S. Smith & Son, London.....	1896–1	Double overcoil, s.r., g.b., "Karrusel"....	+2·7	+1·6	+2·5	+3·3	+3·4	0·5	0·06	3·7	30·9	37·9	16·3	85·1
,, ,, ,, ...	1896–2	Single overcoil, s.r., g.b., "Karrusel".....	−0·2	−1·3	−1·2	+2·4	+3·1	0·3	0·02	5·7	33·3	33·0	18·3	84·6
Evan Roberts, London	19833	Single overcoil, s.r., g.b., "Karrusel".....	+0·6	+1·6	+1·2	+1·1	+3·6	0·5	0·04	5·3	30·1	36·8	17·5	84·4
Jos. White & Son, Coventry	35451	Single overcoil, s.r., g.b., "Karrusel"	−0·3	−0·8	−0·7	−0·4	+0·7	0·5	0·06	4·5	29·8	38·4	16·2	84·4
A. & N C.S., Limited, London ...	2444	Cylindrical, fusee, chronometer.....	+2·0	+4·7	+1·6	+4·2	+3·8	0·4	0·03	5·0	31·2	35·2	17·7	84·1
A. E. Fridlander, Coventry	25507	Single overcoil, s.r., g.b., "Karrusel".....	+4·4	+4·3	+5·0	+2·9	+4·9	0·5	0·06	6·7	30·5	37·7	15·9	84·1

Table I—*continued.*

Watch deposited by	Number of watch.	Balance spring, escapement, &c.	Mean daily rate.					Mean variation of daily rate. ±	Mean change of rate for 1° F.	Difference between extreme gaining and losing rates.	Marks awarded for			Total Marks.
			Pendant up.	Pendant right.	Pendant left.	Dial up.	Dial down.				Daily variation of rate.	Change of rate with change of position.	Temperature compensation.	
											0—40	0—40	0—20	0—100.
			secs.	secs.	secs.	secs.	secs.	secs.	secs.	secs.				
Newsome & Co., Coventry	129635	Single overcoil, s.r., g.b., "Karrusel"	+4·5	+3·0	+3·3	+3·1	+5·1	0·5	0·03	5·7	29·3	36·8	17·9	84·0
S. Smith & Son, London	1896-5	Double overcoil, s.r., g.b., "Karrusel"	+3·3	+2·7	+3·6	+3·0	+3·4	0·5	0·08	6·0	30·0	38·9	14 9	83·8
U. Montandon-Robert, Geneva	1046	Single overcoil, d.r., g.b.	+0·1	−0·2	+3·0	+1·1	+0·9	0·5	0 04	5·7	30·0	36·9	17·0	83·6
Evan Roberts, London	19198	Single overcoil, s.r., g.b., "Karrusel"	+5·5	+5·7	+5·6	+1·6	+5·6	0·5	0·02	7·0	30·1	35·0	18·4	83·5
C. J. Hill, Coventry	149623	Single overcoil, s.r., g.b., "Karrusel"	+2·4	+2·3	+2·6	+4·6	+5·4	0·4	0·05	5·2	31·5	35·2	16 7	83·4
J. Kellie, Liverpool	88 / 26960	Single overcoil, s.r., g.b., "Karrusel"	+0·6	+0·5	+0·4	+4·0	+6·0	0·4	0·01	6·7	32·9	31·4	19·0	83·3
C. J. H. Marlow, Coventry	19907	Single overcoil, s.r., g.b., "Karrusel"	+2·1	+1·8	+2·1	+4·2	+6·4	0·4	0·04	6·0	32·4	33·6	17·3	83·3
T. Russell & Son, Liverpool	89597	Single overcoil, s.r., g.b., "Karrusel"	+3·9	+5·4	+4·3	+3·9	+4·9	0·5	0·05	4·5	29·2	37·8	16·3	83·3
Carley & Co., London	49906	Single overcoil, s.r., g.b., "Karrusel"	+3·5	+3 5	+3·1	+5·2	+4·2	0·5	0·06	6·2	29·9	37·4	15·8	83·1
B. Bonniksen, Coventry	19973	Single overcoil, s.r., g.b., "Karrusel"	−0·4	−0·9	−0·5	−1·9	−1·3	0·6	0·05	6·7	28·1	38·1	16·8	83·0
W. Matthews, Coventry	34139	Single overcoil, s.r., g.b.	−0·5	−2 2	−4·0	−3·0	−1·2	0·6	0·03	6·5	28·9	35·8	18·2	82·9
Jos. White & Son, Coventry	35118	Single overcoil, s.r., g.b.	+1·9	−0·6	+1·2	−0·7	+1·9	0·5	0·04	6·0	30·0	35·5	17·2	82·7
Newsome & Co., Coventry	125069	Single overcoil, s.r., g.b., "Karrusel"	−3·6	−4·8	−4·4	−1·8	+1·2	0·4	0·03	8·2	31·8	32·4	18·3	82·5
,, ,, ,,	129113	Single overcoil, s.r., g.b., "Karrusel"	+1·8	+2·8	+3·1	+2·6	+0·9	0·4	0 07	5·7	31·1	34·0	17·4	82·5
Jos. White & Son, Coventry	35161	Single overcoil, s.r., g.b.,	+2·3	+2·2	+0·4	+1·3	+2·7	0·6	0·04	5·5	28·3	37·0	17·2	82·5
J. Adams, Coventry	6314	Single overcoil, s.r., g.b., "Karrusel"	+1·8	+0·1	+0·5	−0·1	+2·4	0·6	0·02	7·7	27·8	36·2	18·5	82·5
A. E. Fridlander, Coventry	25508	Single overcoil, s.r., g.b., "Karrusel"	−0·5	−1·9	−1·3	+0·6	−1·2	0·6	0·05	4·0	29·0	37·0	16·4	82·4
,, ,, ,,	25509	Single overcoil, s.r., g.b., "Karrusel"	+1·1	+1·2	+0·4	+3·8	+4·5	0·4	0·04	6·0	31·2	33·8	17 3	82·3
S. Yeomans, Coventry	73564	Single overcoil, s.r., g.b., "Karrusel"	+3·3	+3·1	+3·2	+2·6	+2·3	0·6	0·08	7·5	28·9	38·6	14·7	82·2
,, ,, ,,	73561	Single overcoil, s.r., g.b., "Karrusel"	−0·6	+0·3	+0·1	−2·2	+0·3	0·7	0·02	6·0	26·8	36·9	18·5	82·2
A. & N.C.S., Limited, London	7163	Single overcoil, s.r., g.b., "Karrusel"	−1·9	−1·6	−1·8	−1·5	−2·4	0·7	0·03	4·7	25·0	38·9	18·3	82·2
Newsome & Co., Coventry	125138	Single overcoil, s.r., g.b., "Karrusel"	+4·3	+3·1	+3·9	+2·9	+6·2	0·6	0·03	9·0	27·7	36·3	18·1	82·1
Evan Roberts, London	19834	Single overcoil, s.r., g.b., "Karrusel"	−4·4	−5·4	−5·8	−2·8	−7·5	0·6	0·03	6·5	28·9	35·0	18-0	81·9
W. Matthews, Coventry	34855	Single overcoil, s.r., g.b., "Karrusel"	−4·2	−3·1	−4·2	−6·5	−2·7	0·6	0·03	7·2	27·6	36·0	18·3	81·9
A. E. Fridlander, Coventry	52900	Single overcoil, s.r., g.b., non-magnetic	−7·7	−7·9	−7·3	−6·2	−7·0	0·4	0·12	7·5	31·5	38·0	12·3	81·8
C. J. H. Marlow, Coventry	19595	Single overcoil, s.r., g.b., "Karrusel"	0·0	+0·7	+1·5	−0·4	+4·1	0·4	0·06	7·2	31·0	34·9	15·9	81·8
J. Hewitt, Coventry	53085	Single overcoil, s.r. g.b., "Karrusel"	+4·8	+4·3	+5·2	+3·4	+2·6	0·6	0·06	6·5	28·9	36·6	16·3	81·8
Coventry Co-operat. Watch Soc.	54413	Single overcoil, s.r., g.b., "Karrusel"	+4·2	+3·9	+5·0	+1·5	+4·4	0·6	0·05	8·5	28·6	36·4	16·8	81·8
Thomas Hill & Co., Coventry	2665 / 65319	Single overcoil, s.r., g.b., "Karrusel"	+4·2	+3·2	+4·3	+5·8	+6·1	0·4	0·09	4·7	31·9	36·0	13·8	81·7
Thos. Russell & Son, Liverpool.	89574	Single overcoil, s.r., g.b., "Karrusel"	+3·1	+3·8	+3·5	−0·3	+4·4	0·5	0·04	9·0	29·5	34·9	17·3	81·7
Jos. White & Son, Coventry	35581	Single overcoil, s.r., g.b.	+1·6	+1·3	+1·0	+2·3	+2·2	0·6	0·06	5·3	27·5	38·1	16·1	81·7

Thos. Russell & Son, Liverpool. .	89574	Single overcoil, s.r., g.b., "Karrusel"	+3·1	+3·8	+3·5	−0·3	+4·4	0·5	0·06	5·3	27·5	38·1	16·1	81·7
Jos. White & Son, Coventry	35581	Single overcoil, s.r., g.b.	+1·6	+1·3	+1·0	+2·3	+2·2	0·6	0·06					
Denny Bros., Coventry	25100	Single overcoil, s.r., g.b., "Karrusel"	+2·9	+3·6	+3·9	+3·0	+7·8	0·5	0·04	8·5	30·1	34·3	17·1	81·5
Newsome & Co., Coventry	127070	Single overcoil, s.r., g.b., "Karrusel"	−1·5	−1·0	−2·4	+1·2	−0·5	0·6	0·03	5·0	27·4	36·2	17·9	81·5
D. Buckney, London	11944	Double overcoil, d.r., g.b.	−0·2	+2·0	+4·5	+0·8	−1·1	0·5	0·03	7·5	29·9	33·4	18·1	81·4
A. E. Fridlander, Coventry	14156	Single overcoil, s.r., g.b.	+1·3	−3·4	+0·7	+1·6	−0·3	0·6	0·02	7·0	28·8	34·2	18·4	81·4
" " "	25501	Single overcoil, s.r., g.b., "Karrusel"	+0·1	−2·7	−1·3	+2·8	−0·4	0·4	0·08	7·0	32·5	34·4	14·4	81·3
Jos. White & Son, Coventry	35757	Single overcoil, s.r., g.b.	+0·5	+0·7	−0·7	+1·3	+2·6	0·5	0·07	7·0	29·3	36·5	15·4	81·2
J. Adams, Coventry	6316	Single overcoil, s.r., g.b., "Karrusel"	+2·3	+1·8	+2·9	+0·3	+3·3	0·7	0·02	9·7	26·2	36·6	18·4	81·2
A. & N.C.S., Limited, London	6340	Single overcoil, d.r., g.b., chronograph	−1·8	−0·9	−3·4	+0·5	−1·1	0·6	0·06	5·7	29·0	36·0	16·1	81·1
	88													
J. Kellie, Liverpool	86923	Single overcoil, s.r., g.b. "Karrusel"	−1·3	−0·9	−0·2	+0·3	+1·0	0·6	0·07	5·8	28·3	37·2	15·6	81·1
J. Adams, Coventry	463682	Single overcoil, s.r., g.b., "Karrusel"	+3·3	+2·6	+2·1	+3·2	+4·9	0·7	0·04	5·0	26·2	37·2	17·7	81·1
Rotherham & Sons, Coventry	21648	Single overcoil, s.r., g.b.	+4·5	−0·5	+0·6	+2·9	+1·7	0·5	0·04	7·5	30·0	34·0	17·0	81·0
A. E. Fridlander, Coventry	14415	Single overcoil, s.r., g.b.	−2·7	−4·2	−3·0	−0·9	−2·0	0·5	0·07	6·0	29·4	36·4	15·2	81·0
A. M. Jacobs and Co., London	11935	Single overcoil, s.r., g.b., "Karrusel"	−4·6	−5·2	−4·8	−1·4	−6·5	0·5	0·05	6·2	29·3	35·0	16·7	81·0
S. Yeomans, Coventry	73566	Single overcoil, s.r., g.b., "Karrusel"	+4·4	+3·4	+4·1	+3·6	+8·3	0·6	0·03	7·5	28·9	34·4	17·7	81·0
Wright & Craighead, London	8072	Single overcoil, s.r., g.b., "Karrusel"	−4·2	−5·1	−3·1	−2·4	−1·2	0·6	0·04	6·5	28·3	35·4	17·3	81·0
Usher & Cole, London	28369	Single overcoil, s.r., g.b.	−3·8	+1·5	−4·8	−3·6	−3·8	0·5	0·03	8·0	30·1	33·0	17·8	80·9
A. E Fridlander, Coventry	14113	Single overcoil, s.r., g.b.	−2·3	−0·3	+1·0	−2·4	−5·0	0·5	0·03	8·2	29·7	33·1	18·1	80·9
H. Golay, London	1896	Single overcoil, s.r., g.b., "Karrusel," chronograph	+3·4	+3·6	+2·1	+0·9	+0·9	0·6	0·06	7·2	28·9	35·7	16·3	80·9
A. Taylor, London	7556	Single overcoil, d.r., g.b., annular "tourbillon"	+0·1	−0·8	+0·3	+1·9	+1·5	0·6	0·05	4·8	28·1	36·5	16·3	80·9
T. Russell & Son, Liverpool	89573	Single overcoil, s.r., g.b., "Karrusel"	+0·1	+1·0	+1·9	+3·1	+0·6	0·5	0·10	6·0	30·8	36·4	13·6	80·8
J. Adams, Coventry	464319	Single overcoil, s.r., g.b., "Karrusel"	−2·4	−2·6	−3·8	−3·4	−1·6	0·6	0·06	7·5	27·6	37·3	15·9	80·8
T. Russell & Son, Liverpool	90191	Single overcoil, s.r., g.b., "Karrusel"	+4·2	+3·2	+4·8	+4·6	+7·0	0·6	0·05	6·5	27·5	36·4	16·9	80·8
Jos. White & Son, Coventry	35439	Single overcoil, s.r., g.b., "Karrusel"	+2·8	+3·0	+3·6	+3·1	+5·4	0·7	0·03	5·5	25·5	37·0	18·3	80·8
A. E. Fridlander, Coventry	25503	Single overcoil, s.r., g.b., "Karrusel," non-magnetic	+1·9	0·0	+0·5	+1·6	+5·5	0·4	0·09	7·2	32·7	34·2	13·8	80·7
" " "	52764	Single overcoil, d.r., g.b.	+2·4	+5·0	+2·1	+2·7	+0·7	0·4	0·10	7·2	31·7	35·9	13·1	80·7
J. R. Harris, London "	10294	Single overcoil, d.r., g.b.	+0·7	−0·2	−2·8	+1·3	+4·3	0·5	0·05	8·2	30·9	33·1	16·7	80·7
J. Player & Son, Coventry	18075	Single overcoil, d.r., fusee	+5·1	+7·3	+2·1	+7·0	+5·1	0·5	0·05	7·5	30·1	34·2	16·3	80·6
Jos. White & Son, Coventry	33581	Single overcoil, d.r., g.b.	+0·9	−2·0	+0·8	+1·6	+3·4	0·6	0·05	7·0	28·9	35 0	16·7	80·6
Newsome & Co., Coventry	127069	Single overcoil, s.r., g.b., "Karrusel"	+7·8	+6·6	+6·3	+6·0	+6·1	0·6	0·09	9·0	28·5	38·0	14·1	80·6
Jos. White & Son, Coventry	35792	Single overcoil, d.r., g.b.	+2·7	+1·3	+1·9	+3·2	+7·3	0·4	0·06	7·7	31·4	33·4	15·7	80·5
" " "	35508	Single overcoil, s.r., g.b.	−0·6	−0·7	−0·8	−0·2	+2·7	0·6	0·05	7·5	28·0	35·8	16·7	80·5
Usher & Cole, London	28398	Single overcoil, s.r., g.b., "Karrusel"	+3·0	+2·5	+2·7	+3·5	+3·7	0·7	0·07	7·2	26·8	38·4	15·3	80·5
A. E. Fridlander, Coventry	14110	Single overcoil, s.r., g.b.	−0·6	−2·7	−0·7	0·0	+4·2	0·5	0·05	8·5	30·6	33·3	16·5	80·4
S. Yeomans, Coventry	73563	Single overcoil, s.r., g.b., "Karrusel"	+0·8	−0·1	+0·8	+2·2	+3·4	0·5	0·07	6·0	29·5	35·6	15·3	80·4
C. J. H. Marlow, Coventry	19594	Single overcoil, s.r., g.b., "Karrusel"	−0·5	−0·7	−0·3	+0·4	+5·3	0·6	0·02	7·5	28·9	32·8	18·7	80·4
Jos. White & Son, Coventry	35242	Single overcoil, d.r., g.b.	+1·4	−1·9	−0·6	+0·6	−2·5	0·6	0·03	8·5	27·8	34·9	17·7	80·4
A. E. Fridlander, Coventry	13763	Single overcoil, s.r., g.b.	+0·7	+3·8	−5·3	−0 7	−1·2	0·5	0·02	10·2	30·5	31·1	18·7	80·3
Newsome and Co., Coventry	125328	Single overcoil, s.r., g.b., "Karrusel"	−0·4	+0·3	+0·7	+4·5	+1·3	0·4	0·09	6·0	31·4	35·0	13·8	80·2
J. Player & Son, Coventry	25861	Single overcoil, d r., g.b.	+4·1	+5·8	+1·3	−0·3	+0·7	0·5	0·02	7·2	30·1	31·6	18·5	80·2
Jos. White & Son, Coventry	35515	Single overcoil, s.r., g.b., "Karrusel"	+5·5	+6·1	+5·8	+3·9	+7·4	0·5	0·10	8·5	30·0	36·7	13·5	80·2
" " "	35779	Single overcoil, s.r., g.b., "Karrusel"	+3·2	+2·8	+1·9	+3·2	+7·4	0·5	0·07	8·2	30·9	34·1	15·1	80·1
S. Yeomans, Coventry	73559	Single overcoil, s.r., g.b., "Karrusel"	−0·7	−0·9	−1·1	+1·2	+0·9	0·7	0·04	5·8	26·7	36·2	17·2	80·1

In the above List, the following abbreviations are used, viz. :—s.r. for single roller; d.r. for double roller; g.b. for going barrel; + for gaining rate; − for losing rate.

Table II.

Highest Marks obtained by Complicated Watches during the year.

Description of watch.	Number.	Received from.	Marks awarded for			Total marks,
			Variation.	Position.	Temperature.	
			0—40	0—40	0—20	0—100.
Minute chronograph and repeater (minute)	2199	S. Smith and Son, London	28·5	29·7	13·8	72·0
,, ,, ,, (quarter)....	1957	L. Rozat, Chaux-de-Fonds	23·7	28·1	18·9	70·7
Minute and split seconds chronograph	1896	H. Golay, London	28·9	35·7	16·3	80·9
,, ,, ,, ...	60291	Antoine Frères, Besançon.....	28·7	33·7	16·4	78·8
,, ,, ,, ...	3250	Baume and Co., London	27·2	33·4	17·1	77·7
,, ,, ,, ...	3146	S. Smith and Son, London	23·8	32·2	16·4	72·4
Minute and seconds chronograph	6340	A. & N. C. S., Limited, London	29·0	36·0	16·1	81·1
,, ,, ,, ...	166277	Stauffer, Son, and Co., London	25·2	33·1	17·1	75·4
,, ,, ,, ...	147416	,, ,, ,,	25·2	34·1	14·1	73·4
Minute repeater	48900	Carley and Co., London	29·5	34·9	11·1	75·5
,, ,, ...	3418	H. Golay, London	30·3	31·9	12·4	74·6
,, ,, ...	38361	Clemence Frères, London.....	27·8	31·0	14·6	73·4
"Non-magnetic" watches.....	52900	Fridlander, Coventry	31·5	38·0	12·3	81·8
,, ,, ...	25503	,, ,, ...	32·7	34·2	13·8	80·7
,, ,, ...	52838	,, ,, ...	27·8	37·8	12·3	77·9

Report of Magnetical Observations at Falmouth Observatory for the Year 1896. Latitude 50° 9′ 0″ N. and Longitude 5° 4′ 35″ W.; height, 167 feet above mean sea-level.

These observations have been made by instruments purchased from the Government Grant Fund administered by the Royal Society.

Photographic curves of Magnetic Declination and of Horizontal Force variations have been regularly taken throughout the past year, with the exception of three days in December, and the magnets have worked satisfactorily throughout.

The results obtained from the Vertical Force Magnet are not sufficiently reliable for publication.

The scale values of the instruments were determined on 1st January, 1897. The following values of the ordinates of the photographic curves were then found:—

Declination, 1 cm. = 0° 11′·7.
Bifilar, 1 cm. δ H. = 0·00050 C.G.S. unit.

The principal magnetic disturbances recorded during the year occurred on the following dates:—January 3, 4, 5, 31; February 2, 4, 28; March 4, 26, 27, 28; April 22, 23, 24; May 2, 3, 17, 18, 20; June 16; July 3, 4, 5; August 1, 2; September 18; October 11, 12; December 3, 4.

Observations with the Absolute Instruments have been made monthly, of which the following is a summary:—

Determinations of	Horizontal Intensity,	34.
,,	Inclination,	34 sets of four.
,,	absolute Declination,	34.

Following the method adopted in the five previous years, it is intended that the observations be reduced, and that the Declination and Horizontal Force curves for five quiet days in each month of the year—selected by the Astronomer Royal—be tabulated and prepared for publication, in accordance with the International scheme. The results will be printed in the Royal Cornwall Polytechnic Society's Annual Report, and also in the 'Proceedings' of the Royal Society.

The following are the principal results of the magnetic elements for the year 1896:—

Mean Westerly Declination,	18° 47′·5.
Mean Inclination,	67° 5′·0.
Mean Horizontal Force,	0·18554 C.G.S. unit.

The Declination and Horizontal Force are deduced from hourly readings of the photographic curves, and so are corrected for the diurnal variation.

The Inclination is the mean of the absolute observations, the mean time of which is 3 P.M.

In Table V, X is the mean of the absolute values observed during the month (generally three in number), uncorrected for diurnal variations and for any disturbance. Y is the mean of the products of the Dips and X.

The results in the following tables, Nos. I, II, III, IV, are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet marked 66A, and the Declinometer Magnet marked 66C in the Unifilar Magnetometer (No. 66) by Elliott Brothers, of London. Table No. V is deduced from these observations. The temperature correction (which is probably very small) has not been applied.

The Inclination was observed with the Inclinometer by Dover, of Charlton, Kent, No. 86, and needles 1 and 2, which are $3\frac{1}{2}$ ins. in length, the results of which appear in Table VI.

The Declination and Horizontal Force values given in Tables I to IV are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing magnetic observations, and the time given is Greenwich mean time, which is 20 min. 18 sec. earlier than local time.

The following is a list of the days during the year 1896 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables:—

January	1, 2, 21, 24, 29.
February	7, 18, 20, 23, 24.
March	11, 16, 17, 18, 21.
April.....	7, 14, 16, 20, 30.
May	5, 6, 9, 26, 29.
June	2, 7, 20, 23, 24.
July	2, 9, 17, 19, 31.
August.....	5, 13, 16, 27, 28.
September	8, 9, 10, 25, 28.
October.....	6, 7, 18, 25, 26.
November.....	3, 12, 22, 24, 25.
December.....	8, 12, 18, 19, 24.

The whole of the instruments have been maintained in good order. The Magnetic Hut in the garden has been painted inside and out, and the Magnetic Chamber thoroughly drained, to prevent the

recurrence of flooding during periods of excessive rainfall. The Photographic curves were suspended for four days in November owing to the presence of workmen in the chamber.

The Committee appointed by the British Association in 1895 to make a comparison of the Magnetic Standard Instruments in use at the several Magnetic Observatories in the Kingdom presented their Report at the Liverpool meeting of the Association, held August, 1896. Referring to Falmouth, the Committee, of whom Professor A. W. Rücker, M.A., F.R.S., was chairman, state *inter alia*:—"The work of the Falmouth Observatory is hampered by want of funds. The Vertical Force recording instrument has never worked properly, and appears to want extensive alterations. The Observations made by the Superintendent, Mr. E. Kitto, are of a very high order of excellence, and it is to be hoped that the Royal Cornwall Polytechnic Society, by which the Observatory was founded, will be able to ensure the maintenance of the Magnetic Observations under the best conditions."

EDWARD KITTO,
Magnetic Observer.

Table I.—Hourly Means of Declination at the Falmouth on five selected quiet Days in

(18° + West.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
Winter.												
1896.	′	′	′	′	′	′	′	′	′	′	′	′
*Jan. ..	49·0	49·4	49·6	49·6	50·2	50·2	50·0	49·7	49·2	48·7	49·5	51·1
Feb. ..	48·5	49·1	49·2	49·1	49·1	49·2	49·1	48·9	48·5	48·7	49·2	50·3
March .	48·9	48·7	48·9	48·6	48·4	48·4	48·4	48·0	46·6	45·8	47·1	48·9
Oct. ..	43·5	43·5	43·7	43·6	43·6	43·5	43·4	42·6	42·0	41·9	43·6	46·0
†Nov. ..	42·9	42·9	43·3	43·5	43·9	43·7	43·0	43·0	42·7	41·9	42·9	44·1
Dec. ..	44·2	44·6	44·9	45·0	45·4	45·4	45·1	45·0	44·9	44·8	44·9	45·8
Means	46·2	46·4	46·6	46·6	46·8	46·7	46·5	46·2	45·7	45·3	46·2	47·7
Summer.												
	′	′	′	′	′	′	′	′	′	′	′	′
April..	47·5	47·9	48·1	48·0	47·6	47·8	46·5	45·0	43·8	43·4	44·7	47·4
May ..	48·8	48·6	48·5	48·2	47·6	46·7	45·5	44·7	44·0	44·4	46·4	48·4
June ..	45·9	45·8	45·3	45·1	44·7	43·6	42·3	41·8	42·1	42·9	44·5	46·6
July ..	47·5	47·4	47·2	47·1	46·6	45·7	44·5	43·7	43·8	44·4	46·5	48·6
Aug. ..	46·9	46·9	46·8	46·5	46·3	45·6	45·1	44·3	44·1	44·6	46·8	49·5
Sept. ..	45·1	45·2	45·2	44·9	44·8	44·2	43·5	42·6	41·3	42·0	45·0	48·2
Means	47·0	47·0	46·9	46·6	46·3	45·6	44·6	43·7	43·2	43·6	45·7	48·1

* Mean of four days, 1st, 21st, 24th, 29th. † Mean of four days, 3rd, 22nd, 24th, 25th.

Table II.—Solar Diurnal Range of the Falmouth

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
Summer mean.												
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·7	−0·7	−0·8	−1·1	−1·4	−2·1	−3·1	−4·0	−4·5	−4·1	−2·0	+0·4
Winter mean.												
	′	′	′	′	′	′	′	′	′	′	′	′
	−1·1	−0·9	−0·7	−0·7	−0·5	−0·6	−0·8	−1·1	−1·6	−2·0	−1·1	+0·4
Annual mean.												
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·9	−0·8	−0·8	−0·9	−1·0	−1·4	−2·0	−2·6	−3·1	−3·1	−1·6	+0·4

NOTE.—When the sign is +, the magnet

Observatory determined from the Magnetograph Curves each Month during the Year 1896.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
					Winter.							
′	′	′	′	′	′	′	′	′	′	′	′	′
52·6	53·6	53·6	52·2	50·8	50·1	50·2	49·9	49·7	49·4	49·2	48·5	48·5
52·1	53·2	53·8	53·4	52·2	51·6	50·8	49·8	49·9	49·1	49·0	48·5	48·5
51·9	53·9	54·3	54·0	52·5	50·8	50·4	50·4	50·0	49·8	49·6	49·5	49·5
48·7	49·6	49·0	47·7	46·3	45·4	45·2	44·9	44·6	44·4	44·4	44·0	43·9
45·9	46·5	46·3	45·4	44·7	44·2	43·9	43·5	43·3	43·1	42·9	43·0	42·9
46·6	47·1	46·7	46·2	45·7	45·0	44·9	44·6	44·4	44·2	44·1	44·0	44·0
49·6	50·7	50·6	49·8	48·7	47·9	47·6	47·2	47·0	46·7	46·5	46·3	46·2
					Summer.							
′	′	′	′	′	′	′	′	′	′	′	′	′
51·4	54·0	55·0	53·8	52·6	50·9	49·7	49·2	49·0	48·9	48·7	48·2	47·9
51·3	53·2	54·1	53·5	52·5	51·4	50·3	49·8	49·5	49·1	48·9	48·5	48·6
50·0	51·3	51·8	50·7	49·9	48·9	48·3	48·0	47·9	47·3	46·9	46·9	46·3
51·5	53·0	54·0	53·0	51·2	49·7	49·1	48·5	48·1	48·1	48·2	47·7	47·5
52·6	54·2	54·0	52·6	50·5	49·0	47·9	47·6	47·4	47·3	47·1	47·2	46·5
51·7	53·1	52·3	50·5	48·0	46·7	45·7	45·7	45·4	45·3	44·9	45·1	45·1
51·4	53·1	53·5	52·4	50·8	49·4	48·5	48·1	47·9	47·7	47·5	47·3	47·0

Declination as derived from Table I.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
					Summer mean.							
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·7	+5·4	+5·8	+4·7	+3·1	+1·7	+0·8	+0·4	+0·2	0·0	−0·2	−0·4	−0·7
					Winter mean.							
′	′	′	′	′	′	′	′	′	′	′	′	′
+2·3	+3·4	+3·3	+2·5	+1·4	+0·6	+0·3	−0·1	−0·3	−0·6	−0·8	−1·0	−1·1
					Annual mean.							
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·0	+4·4	+4·6	+3·6	+2·3	+1·2	+0·6	+0·2	−0·1	−0·3	−0·5	−0·7	−0·9

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force at Falmouth on five selected quiet Days in

0·18000 + (C.G.S. units.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
Winter.												
1896.												
*Jan. ..	535	537	537	538	541	541	544	547	548	541	534	526
Feb. ..	536	534	534	533	536	539	541	541	541	533	526	519
March .	550	551	551	551	551	553	553	552	546	535	526	528
Oct. ..	570	567	564	564	566	567	567	564	560	552	543	541
†Nov. ..	569	567	566	567	569	573	572	571	569	562	554	554
Dec. ..	561	562	562	561	562	565	567	567	567	565	560	559
Means	554	553	552	552	554	556	557	557	555	548	541	538
Summer.												
April ..	545	545	545	547	547	547	550	546	541	530	520	516
May ..	554	553	552	551	550	549	551	546	540	536	529	529
June ..	567	563	562	561	562	562	556	551	546	543	542	548
July ..	566	564	563	563	563	561	557	554	549	540	538	542
Aug. ..	564	562	561	559	557	556	554	550	541	531	527	530
Sept. ..	564	564	566	564	562	561	560	551	542	530	525	528
Means	560	559	558	558	557	556	555	550	543	535	530	532

* Mean of four days, 1st, 21st, 24th, 29th. † Mean of four days, 3rd, 22nd, 24th, 25th.

Table IV.—Diurnal Range of the Falmouth

(C.G.S. units.)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
Summer mean.												
	+ ·00004	+ ·00003	+ ·00002	+ ·00002	+ ·00001	·00000	− ·00001	− ·00006	− ·00013	− ·00021	− ·00026	− ·00024
Winter mean.												
	+ ·00002	+ ·00001	·00000	·00000	+ ·00002	+ ·00004	+ ·00005	+ ·00005	+ ·00003	− ·00004	− ·00011	− ·00014
Annual mean.												
	+ ·00003	+ ·00002	+ ·00001	+ ·00001	+ ·00002	+ ·00002	+ ·00002	− ·00001	− ·00005	− ·00013	− ·00019	− ·00019

Note.—When the sign is + the

Observatory determined from the Magnetograph Curves, each Month during the Year 1896.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
Winter.												
526	531	536	538	540	542	543	535	544	543	540	537	538
520	525	529	531	529	530	532	536	538	539	541	542	539
533	544	553	555	554	552	552	557	554	555	556	555	555
545	549	554	557	557	561	566	572	575	571	569	569	568
557	563	567	569	570	573	574	574	574	574	572	569	568
558	562	561	561	562	565	565	567	566	566	566	563	556
540	546	550	552	552	554	555	557	559	558	557	556	554
Summer.												
520	530	540	548	552	557	557	556	559	559	558	556	556
531	536	542	550	557	564	569	573	571	567	566	565	564
552	556	563	568	570	572	575	577	579	574	573	573	570
548	550	558	566	566	570	574	575	577	575	574	573	569
540	552	557	561	564	564	564	568	568	569	564	564	567
539	551	560	561	563	566	564	570	572	572	569	570	569
538	546	553	559	562	566	567	570	571	569	567	567	566

Horizontal Force as deduced from Table III.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
Summer mean.												
−·00018	−·00010	−·00003	+·00003	+·00006	+·00010	+·00011	+·00014	+·00015	+·00013	+·00011	+·00011	+·00010
Winter mean.												
−·00012	−·00006	−·00002	·00000	·00000	+·00002	+·00003	+·00005	+·00007	+·00006	+·00005	+·00004	+·00002
Annual mean.												
−·00015	−·00008	−·00003	+·00002	+·00003	+·00006	+·00007	+·00010	+·00011	+·00010	+·00008	+·00008	+·00006

reading is above the mean.

Table V.—Magnetic Intensity. Falmouth Observatory, 1896.

1896.	C.G.S. measure.	
	X or Horizontal force.	Y or Vertical force.
January	0·18519	0·43748
February	0·18520	0·43775
March	0·18517	0·43722
April	0·18526	0·43814
May	0·18544	0·43907
June	0·18563	0·43934
July	0·18567	0·43904
August	0·18530	0·43867
September	0·18547	0·43903
October	0·18554	0·43920
November	0·18558	0·43887
December.	0·18559	0·43928
Means.....	0·18542	0·43859

Table VI.—Observations of Magnetic Inclination. Falmouth Observatory, 1896.

Month.		Mean.		Month.		Mean.	
		°	′			°	′
January	16.....	67	1·4	July	10.....	67	5·4
	23.....	67	3·8		23.....	67	3·7
	31.....	67	4·9			67	4·6
		67	3·4	August	10.....	67	6·1
February	8.....	67	4·9		28.....	67	5·9
	19.....	67	2·7			67	6·0
	27.....	67	3·5	September	5.....	67	5·6
		67	3·7		9.....	67	5·5
March	10.....	67	2·5		30.....	67	6·7
	21.....	67	2·7			67	5·9
	31.....	67	3·3	October	9.....	67	7·6
		67	2·8		27.....	67	4·2
April	9.....	67	2·1		30.....	67	6·0
	21.....	67	5·8			67	5·9
	30.....	67	6·6	November	11.....	67	5·3
		67	4·8		24.....	67	4·3
May	9.....	67	7·1		30.....	67	4·4
	20.....	67	5·5			67	4·7
	30.....	67	6·0	December	10.....	67	6·3
		67	6·2		21.....	67	4·5
June	10.....	67	7·6		29.....	67	6·6
	19.....	67	5·4			67	5·8
	29.....	67	4·2				
		67	5·7				

March 18, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Cultural Evolution of *Cyclamen latifolium* (Sibth.)." By W. T. THISELTON DYER, C.M.G., C.I.E., F.R.S.

II. "On the Conditions which render Absolute the Readings of the Mercurial Thermometer." By S. A. SWORN. Communicated by Professor CLIFTON, F.R.S.

III. "Experiments on the Flame Spectrum of Carbon Monoxide." By W. N. HARTLEY, F.R.S., Royal College of Science, Dublin.

"The Cultural Evolution of *Cyclamen latifolium* (Sibth.)." By W. T. THISELTON DYER, C.M.G., C.I.E., F.R.S. Received and read March 18, 1897.

On the occasion of the discussion on "Variation in Plants and Animals," which took place on February 25, 1895, it occurred to me that it might be useful to give an illustration of the amount of change which had been effected in a plant by continuous selection under cultivation in a comparatively short time. I, therefore, placed upon the table an example of the wild and of the cultivated form of the garden "cineraria" (*C. cruenta*).

The choice of this species was purely accidental. It was, however, violently impugned. It was contended that the garden cineraria was not the result of the development of a single species, but that it was of multiple origin, and the result of the intercrossing of several. It was further contended that its change from the wild form had not been gradual, but by discontinuous steps or "sports." Neither contention seemed to me well founded. But I admit that, owing to the lapse of time since the so-called "improvement" of the cineraria commenced, it is impossible to give formal proof that the process has been what I described. Mr. Darwin met with the same difficulty. He remarks: "We know hardly anything about the origin or history

of any of our domestic breeds."* As is, however, well known, he regarded them as the result of accumulation by selection of successive slight variations. But he also tells us that "the chance will be infinitely small of any record having been preserved of such slow, varying, and insensible changes."

It seemed to me important, therefore, to obtain the history of some cultivated plant which would not be open to the objections urged in the case of the cineraria.

After some consideration I selected the plant known in gardens as *Cyclamen persicum*. Owing to the kindness of the skilful horticulturists who have worked upon it, I am able to place on record a nearly complete history of the changes it has undergone.

The genus *Cyclamen* belongs to the small order *Primulaceæ*, which in its affinities is somewhat isolated. *Cyclamen* itself is distinguished from the rest of the tribe *Lysimachieæ*, to which it belongs, by the reflexed segments of the corolla.

Cyclamen persicum, Mill., is a name given by gardeners to a form slightly modified by cultivation of *C. latifolium*, Sibth., a species confined to Greece and Syria. There is a good figure of the type in Sibthorp's 'Flora Græca' (t. 185). It has pink flowers, with a ring of darker colour at the throat. The species is said to have been first cultivated in Europe at Lille in 1731,† having been introduced from Persia. There must have been some error as to its origin, for Boissier points out that the species is not found in that country.‡ In all probability it was obtained from Syria. The Lille plant ultimately went to Ghent, and it has been asserted that all the cultivated forms in existence are descendants from this one individual. The assertion cannot be proved, but is not improbable. It is known to have been a variety with white flowers. As will be shown, the forms now in cultivation have been derived from a white-flowered one, which in turn might well have been derived from the Lille plant.

Such a modified form was, in fact, that described by Miller, in 1768, in the eighth edition of his 'Gardener's Dictionary,' under the name of *Cyclamen persicum*. He describes the flowers as "pure white with a bright purple bottom." It was figured in the 'Botanical Magazine' in 1787 (t. 44), and it has come down little altered to our own day. In 1875 Boissier describes it as "forma hortensis a me nunquam spontanea visa." It still exists in cultivation, and is the (old) "crimson and white" of Messrs. Sutton. It seems always to have been popular in cultivation on account of its agreeable fragrance. This confirms the Syrian origin of the original stock, for

* 'Origin,' 6th ed., p. 29.

† 'La Semaine Horticole,' 1897, p. 23.

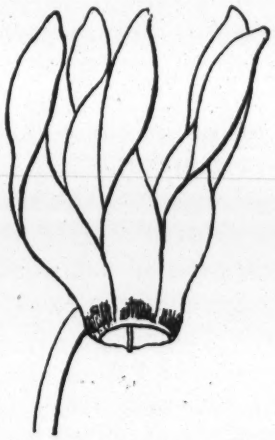
‡ 'Flora Orientalis,' vol. 4, p. 12.

a white-flowered form "is found in Palestine which is very fragrant."*

Early in the century some colour variations were in cultivation. Several as well as the typical *C. persicum* were figured in the 'Flore des Serres' in 1877 (t. 2345). These record the amount of change from the wild type which had been accomplished in a century and a half. One striking seminal sport (*C. persicum*, var. *laciniatum*) is figured in the 'Botanical Register' in 1827 (t. 1095). It is remarkable for spreading corolla-segments broader than usual, and cut at the edges. It does not appear to have been perpetuated, but in some degree it anticipated some of the remarkable modern developments.

I am informed by Mr. James Martin, the accomplished propagator of Messrs. Sutton, that the recent remarkable development of the cyclamen began about 1860, and, at any rate in their hands, started with the old "crimson and white." It will be seen from the accompanying figures how little this differs from the wild type. Fig. 1

Fig. 1.

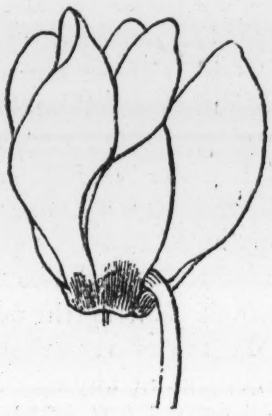


represents a flower of the latter from a plant imported by Messrs. Sutton from Syria after six years of cultivation. It is not appreciably altered. Fig. 2 represents a flower of their "crimson and white;" it only differs from the wild type in having shorter, broader, and less twisted corolla segments.

In considering the progress which has been made since 1860 under the skilful hands of Mr. Martin and others, it is important to bear in mind that there is no question of hybridity. *Cyclamen latifolium* has resisted every attempt to cross it with any other species. We are dealing then with the evolution under artificial

* 'Roy. Hort. Soc. Journ.,' N. S., vol. 13, p. 163.

Fig. 2.



conditions of a single species. Further, in the following statement, I have confined myself to the result of continued self-fertilisation, and have not thought it necessary to investigate the results of crossing races which have assumed characters more or less distinct.

Size.

Mr. Martin strongly insists on the principle laid down by Mr. Darwin from De Vilmorin, that "the first step is to get the plant to vary in any manner whatever."* As Mr. Martin puts it, "the breeder must work with nature." It is his practice to seize the smallest deviation, even so small an indication as the slightest difference in a cotyledon of a germinating seed. The first direction of work would, however, for commercial purposes, be to develop the size of the corolla. Figs. 3 and 4 show two stages which have been reached by progressive selection from "crimson and white." Messrs. Sutton have sent me photographs of the largest flowers hitherto produced by them. Fig. 5 is copied from one of these. The vertical depth is 3 in. This is more than double that of the form with which they started; the increase in breadth of the segments is at least six times. This represents the continuous work of forty years. As the work was not done for a scientific purpose, the whole of the progressive steps have not been preserved or recorded. Only saleable stages have survived. But Mr. Martin emphatically denies that they have been attained by other than progressive selection or that they have been reached by leaps and bounds. In developing any particular character it is, to use his own words, always done by a "ladder," *i.e.*, continuous self-fertilisa-

* 'Animals and Plants under Domestication,' vol. 2, p. 262.

Fig. 3.

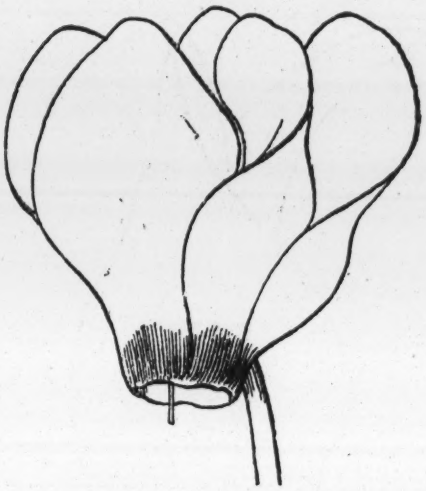
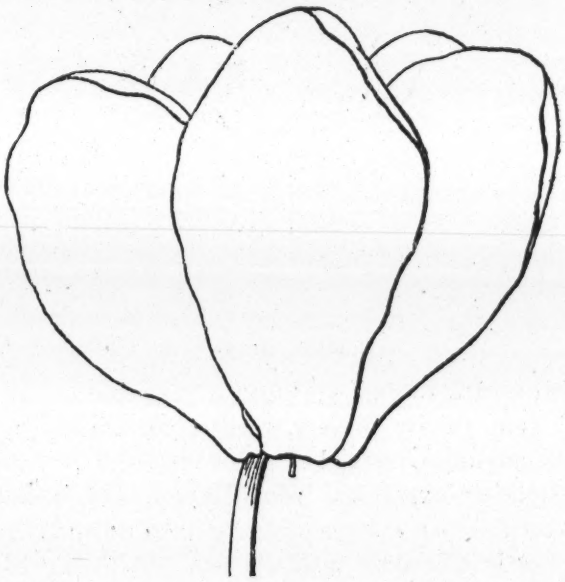


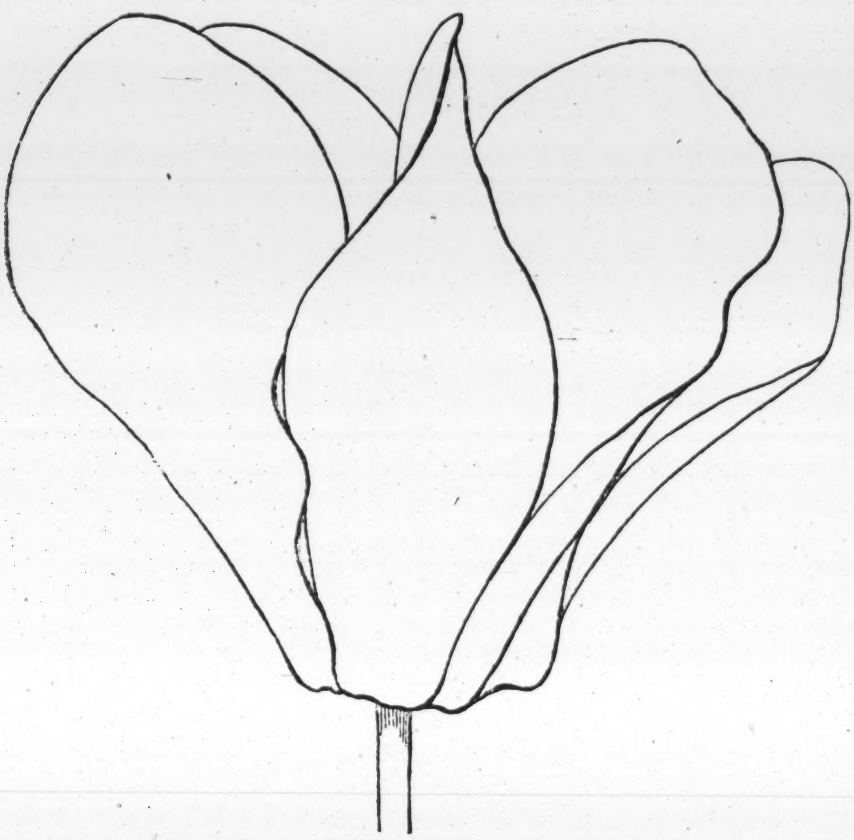
Fig. 4.



tion and selection. The stage shown in fig. 3 owes its preservation to its having retained fragrance. Beyond this stage fragrance has been lost.

An interesting question is whether there is any limit to the extent to which an organ can be developed, and if so, what? It is to be hoped that Mr. Martin will continue his work in this direction and strive, if only as a matter of scientific interest, to increase the size of the corolla to the maximum possible. The only check will probably be found to be the general balance of nutrition.

Fig. 5.



Spreading.

I was much struck to find amongst a magnificent series of specimens, kindly sent me by Messrs. Sutton, forms with the segments of the corolla spreading instead of reflexed (fig. 6). I have received even more striking examples from Messrs. Hugh Low. This is remarkable because, as I have already pointed out, the latter is a distinctive generic character in *Cyclamen*. Although the alteration in the appearance of the flower is enormous, the structural change is slight. It is merely a matter of direction of growth. It amounts, however, to the loss of a generic character and a reversion to a more generalised type. The change is therefore essentially atavistic.

I was unable to obtain from Mr. Martin any explanation of how this particular variation had come about, but he informed me that it had been of frequent occurrence. Spreading flowers had always been destroyed as departing from a desirable type. More recently, on account of their orchid-like habit, they had taken the popular fancy, and had been preserved.

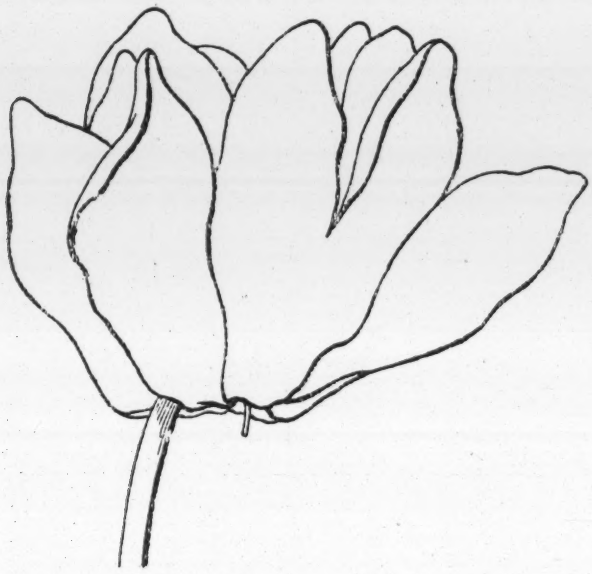
Fig. 6.



Doubling.

Even in the wild type there is a tendency to a slight multiplication of the corolla segments. Mr. Martin has worked upon this, and has produced flowers such as shown in fig. 7. He seems to think that there is no limit to which this multiplication cannot be carried practically, and hopes in time to produce "mop-headed" flowers like a chrysanthemum. The so-called doubling of flowers, as in the rose, is a teratological phenomenon, and is due to the conversion of stamens into petals. But in *Cyclamen* this is not the case. The stamens, which are normally equal in number to the corolla segments, are also multiplied. Although a quinary symmetry is general in the *Primulaceæ*, *Trientalis*, a near ally of *Cyclamen*, ordinarily exhibits a considerable range in the number of parts of the flower. Here again *Cyclamen*, under artificial conditions, shows a reversion to a more generalised type.

Fig. 7.



Colour.

There is evidence that seminal variation as regards colours occurred at least as early as 1820, but the modern forms with large coloured flowers, according to Mr. Martin, originated in a different way and can be traced back to the old crimson and white. That preserves the crimson ring round the throat, but is otherwise an albino. There is nothing remarkable in this. Any species in nature may produce white flowers; albinism is in effect the commonest of all variations. "Giant white" (figs. 4 and 5) is a pure albino, in which the crimson ring has been suppressed.

The modern coloured forms were obtained in the first instance by selecting forms in which the coloured ring showed a disposition to spread into the white corolla segments. The first indication would be a scarcely perceptible streak. By selection from self-fertilised plants the streak was widened into a stripe. Continuing the process, the stripes united, and a uniformly coloured flower was obtained.

The more striking colours, such as that of "Vulcan," which is a dark crimson, were, however, obtained not by progressive selection, but amongst the progeny of cross-fertilised plants.

I learn from Messrs. Hugh Low and Co. that coloured varieties, of course when self-fertilised, come true from seed. This is in accordance with a well-known principle.*

* Darwin, 'Cross and Self-fertilisation,' p. 460.

The Butterfly Form.

This has been obtained independently by several horticulturists. The segments are partially spreading, and concave on their inner surface. One of the most remarkable is that raised by M. de Langhe-Veroaene; it is represented in figs. 8 and 9. He informs me that "these are the products of the eleventh year of improvement." He adds: "I never crossed them with any other strain; I do not like

FIG. 8.

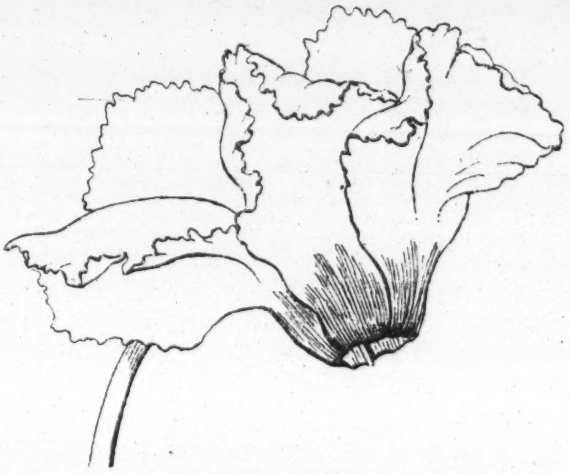
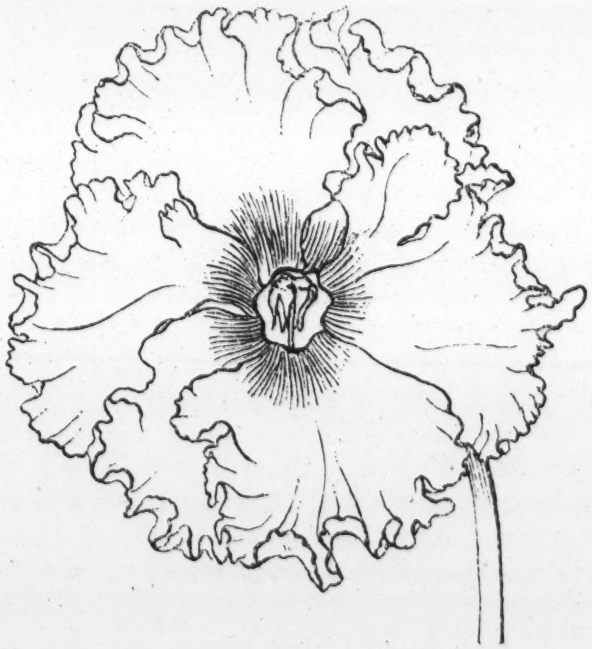


FIG. 9.



crossing races; I prefer improving them." He has kindly favoured me with the following detailed account of the mode in which the strain has been developed and improved. I quote it in his own words:—

"Les Cyclamen Papilio que j'ai obtenus sont issus directement des *Cyclamen persicum*, var. *giganteum*.

"Il y a environ une douzaine d'années je remarquais parmi mes semis de Cyclamen une plante qui attira mon attention par l'extrême beauté de son feuillage dentelé et marbré. En examinant la plante, je vis qu'elle portait une grande quantité de boutons; ceux-ci étaient de forme plus arrondie et plus courte que ne le sont généralement ceux des *Cyclamen persicum*. La plante fut mise à part; quand elle commença à fleurir, elle m'étonna par la forme bizarre de ses fleurs. Ces diverses circonstances m'engagèrent à en recolter les graines.

"L'année suivante j'obtins quelques jeunes plantes. Au moment de leur floraison, elles purent être comparées à la plante mère.

"Les plus parfaites de ces plantes furent choisies pour servir de porte-graine, et leurs fleurs furent fécondées entre elles. L'année suivante je fus assez heureux pour constater un nouveau progrès; mes gains surpassaient leurs parents que j'avais conservés. On pouvait apercevoir, dans ces semis aux caractères persistants, le point de départ d'une race nouvelle.

"Je continuai dans cette voie; au bout de quatre ans, j'étais en possession de quelques sujets fort remarqués. Les pétales des fleurs étaient amples et plus longs; ils se redressaient comme les ailes d'un papillon qui s'apprête à prendre son vol.

"La race se caractérisa chaque année davantage.

"Encouragé par le résultat déjà obtenu, je m'occupai à rechercher la diversité des coloris. Après quatre années je ne possédais dans mes semis que des plantes à fleurs rouges; j'avais en triant les sujets pour la reproduction toujours écarté les fleurs les moins brillantes. Il s'agissait maintenant d'obtenir des fleurs à couleurs pâles. Des efforts nouveaux furent fait dans cette voie; je vis au bout de deux ans après apparaître la première fleur aux pétales blancs et à onglet rouge; dès lors les croisements se multiplièrent au point qu'après la neuvième année la perfection des formes et des coloris est telle que tous ceux qui voient mes Cyclamen Papilio sont unanimes à reconnaître leur mérite et leur perfection des fleurs."

In this case the basis of the new strain was found in a marked variation or "sport." The deviation from the type could not, however, have been very marked. The most remarkable feature in "Papilio" as now developed is the curled and toothed margin of the corolla segments. These peculiarities repeat characters which occur elsewhere in the order. In *Soldanella* the toothing is conspicuous; curling occurs in cultivated varieties of *Primula sinensis*. It is

interesting to observe in "Papilio" that in the primary variation there was a correlation between the toothing of the corolla segments and of the leaves.

Cresting.

The most remarkable form which has made its appearance under cultivation is that in which a plumose crest has developed on the inner surface of each corolla segment. This is shown in fig. 10,

FIG. 10.



which represents the "Bush Hill Pioneer," raised by Messrs. Hugh Low and Co. I quote the account of its development with which they have been so good as to furnish me:—

"This interesting variety was first observed in our nurseries some four years since, but how it originated we are unable to say.

"At that time, the only peculiarity about the variety was a *very slightly raised rib* running part of the way up the petals, and showing *no tendency to branch.* This was, however, considered sufficiently curious to follow up, and we seeded it with its own pollen.

"The young plants from this *showed a decided improvement*, the rib in some cases *showing a marked tendency to branch.* The best varieties (ten in number) were again fertilised with their own pollen, and the plants now being exhibited by us have resulted, although needless to say, they are among the finest obtained up to the present though *all* show a further improvement, *every flower* having a well-branched feather on the petals.

"We have this year found some colour in one plant, and we

believe we shall have no trouble in obtaining crested flowers in a variety of colours."

The corolla segments of *Cyclamen* have no mid-rib. The appearance of such a structure is a reversion to the original leaf-type. The development of a crest from a mid-rib carries reversion very far back indeed. The branching of a leaf-structure in the plane in which it is expanded is common enough; branching in a plane at right angles to this is rare. Leafy outgrowths frequently occur from the mid-rib in the cabbage.* In this case the structure of the leaf approximates to that of a stem, of which, indeed, the leaf may be regarded as a modification.

An interesting fact with regard to this singular variation is that it has appeared more than once, and independently. It first occurred in 1885, but seems afterwards to have been lost sight of.† It has also occurred in a red-flowered form in France,‡ in which case it was also perpetuated by seed.

I have not succeeded in discovering any similar structure in any primulaceous plant occurring in a wild state. Dr. Masters, however, informs me that it has been observed in cultivated forms of *Primula sinensis*. The tendency thus seems to be latent in the order, though why it should be so I am unable to explain.

Some theoretical interest appears to me to attach to the rapid development of so striking an ornament of a corolla segment. Such appendages are frequent enough in orchids, and are regarded as adaptations to cross-fertilisation by insects. Their gradual evolution might be thought to require a long period of time; but in the present case we have definite evidence that such a structure may be developed by selection with great rapidity.

Conclusion.

1. The facts which I have stated appear to me to establish the result that when once specific stability§ has been broken down in a plant, morphological changes of great variety and magnitude can be brought about in a comparatively short space of time. This appears to me to have a very important bearing on the rate of evolution. Mr. Darwin quotes Lord Kelvin as insisting "that the world at a very early period was subjected to more rapid and violent changes in its physical condition than those now occurring;" and he adds, "Such changes would have tended to induce changes at a corre-

* Masters' 'Teratology,' p. 455.

† 'Gardener's Chronicle,' 1885, p. 536.

‡ 'Revue Horticole,' 1897, pp. 98 and 130.

§ For a general discussion of the principles of variation and specific stability, see 'Nature,' vol. 51, pp. 459—461.

sponding rate in the organisms which then existed."* That changes may be effected with considerable rapidity cannot, I think, be denied.

2. It is further, I think, abundantly proved in the present case that, though sudden variations do occur, they are, as far as we know, slight as long as self-fertilisation is adhered to. The striking results obtained by cultivators have been due to the patient accumulation by selection of gradual but continuous variation in any desired direction.

3. The size which any variable organ can reach does not appear to be governed by any principle of correlation. Large flowers are not necessarily accompanied by large leaves. Under natural conditions size is controlled by mechanical limitations and by the principle of economy. Nature cannot afford to indulge in anything unnecessary for the purpose in view.†

4. The general tendency of a plant varying freely under artificial conditions seems to be atavistic, *i.e.*, to shed adaptive modifications which have ceased to be useful, and either to revert to a more generalised type or to reproduce "characters which are already present in other members of the same group."‡ This conclusion must, however, be accepted with caution, for we must remember that in a case like the present we are only acquainted with variations which have been preserved with a particular end in view.

5. The case of "cresting" shows that the plant still possesses the power to strike out a new line and to develop characters which would even be regarded as having specific value, as in the total change which has been effected in the form of the leaf in *Primula sinensis*. If such a race developed any degree of sterility with other races it would have satisfied Huxley's criterion for the artificial production of a new species.

* 'Origin,' 6th ed., p. 286.
† See Darwin, 'Origin,' 6th ed., p. 117.
‡ See Darwin, 'Origin,' 6th ed., p. 127.

March 25, 1897.

The LORD LISTER, F.R.C.S., D.C.L.., President, in the Chair.

Meeting for Discussion.

Subject: "The Chemical Constitution of the Stars."

The following Paper and Notes were read in the course of the discussion :—

"On the Chemistry of the Hottest Stars." By J. NORMAN LOCKYER, C.B., F.R.S. Received March 8, 1897.

[PLATES 1—3.]

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VI. Lines of cleveite gases in cooling stars.
VII. Metallic lines and lines of cleveite gases in cooling stars.

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1. Arc and spark spectra of magnesium, iron, and calcium.
2. Photographic spectra of typical stars of increasing temperature.
3. Photographic spectra of typical cooling stars.

I. Introduction.

The study of stellar spectra from the time of Rutherfurd to the present shows us that only a very small number of groups is in question. We seem to be in presence of an evolution in which only a very few variables are in operation, and in my opinion the phenomena suggest that the only variable of paramount importance is temperature.

In working out the classification of stellar spectra, which I communicated to the Royal Society in 1888, the course pursued was to study the flutings and lines of the various elements given in the existing lists and to fill up gaps in them by fresh experimental work with the view of finding the necessary criteria.

The question, however, was complicated by the discovery in stellar spectra of many lines, the origin of which could not be stated.

Some time has now elapsed since my classification was published. In the meanwhile the attempts to trace the origin of the unknown lines have been continued, and the discovery of terrestrial sources of helium, and probably other gases, has thrown a flood of new light upon stellar chemistry.

I propose in the present paper to trace the history of the criteria now at our disposal in the study of the phenomena of the hottest stars and to give the results of my latest researches. In the present communication, thus limited in scope, we are only concerned with spectral line phenomena, and it is important that some of the main points connected with line spectra in their relation to celestial bodies should be referred to by way of introduction to the later results.

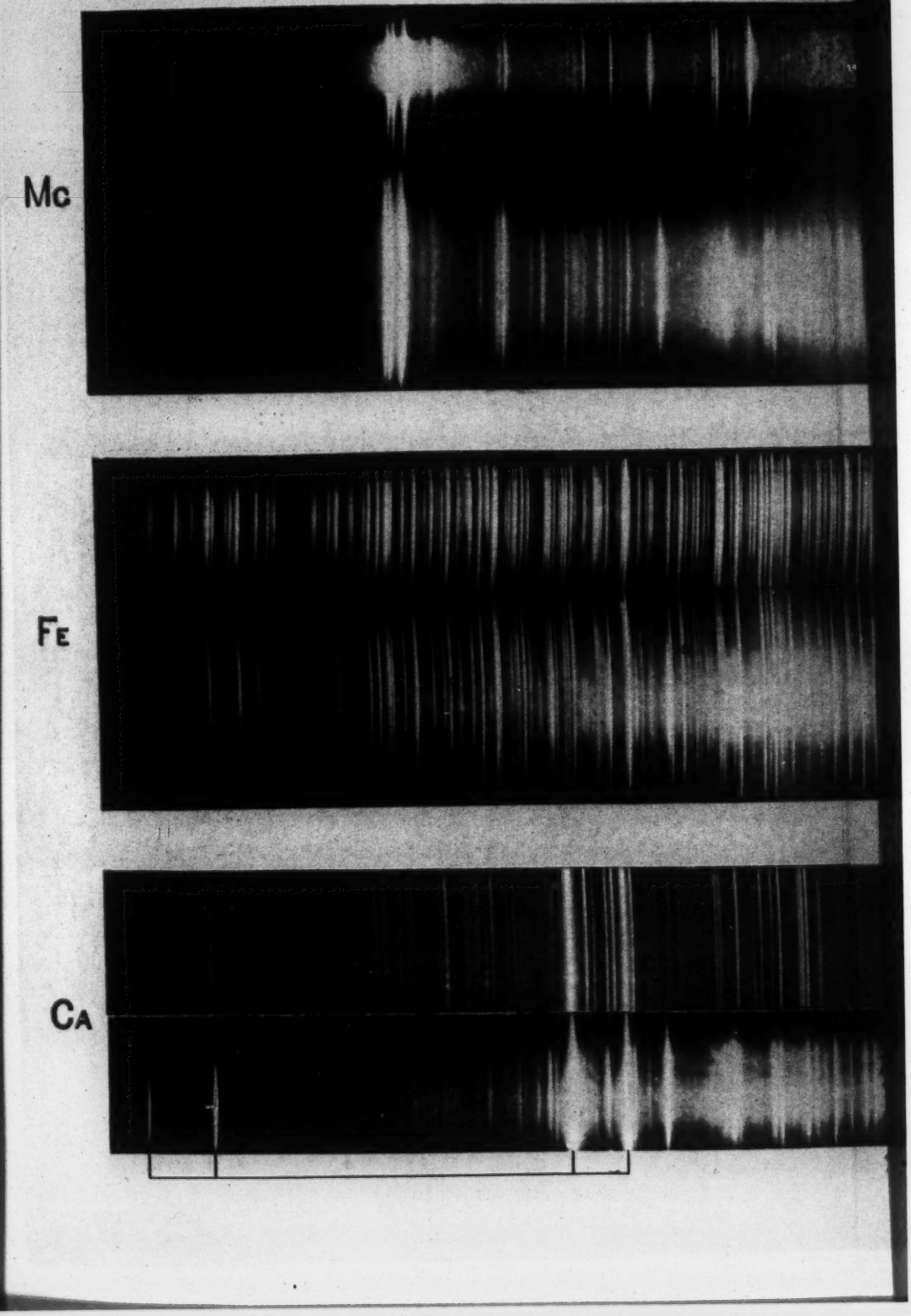
The Criterion of Long and Short Lines.

In November, 1872,* I showed that when an image of an electric spark is thrown on the slit of a spectroscope the lines are seen of

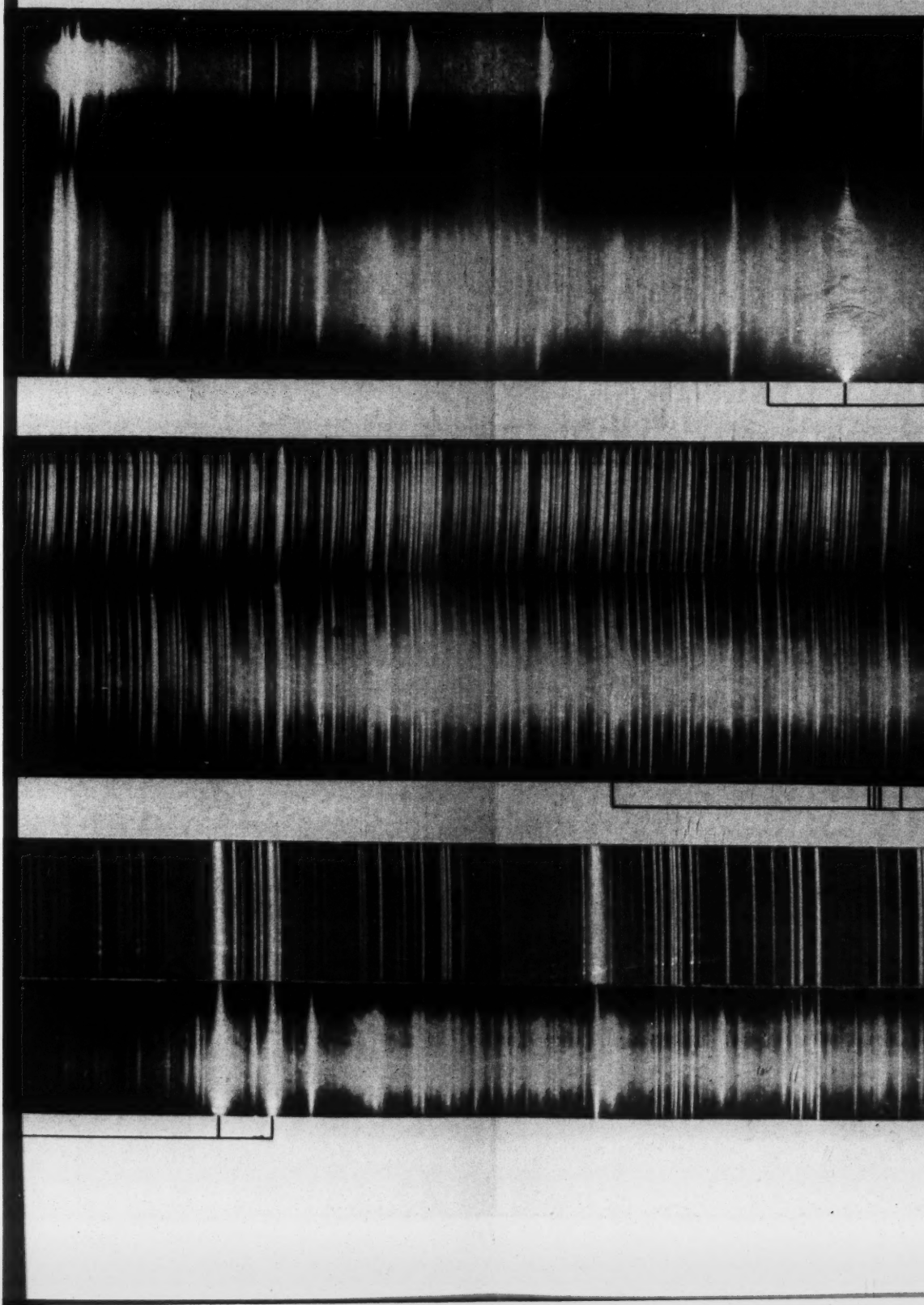
* 'Phil. Trans.,' 1873, vol. 163, Part 1, pp. 253-275.

LOCKYER.

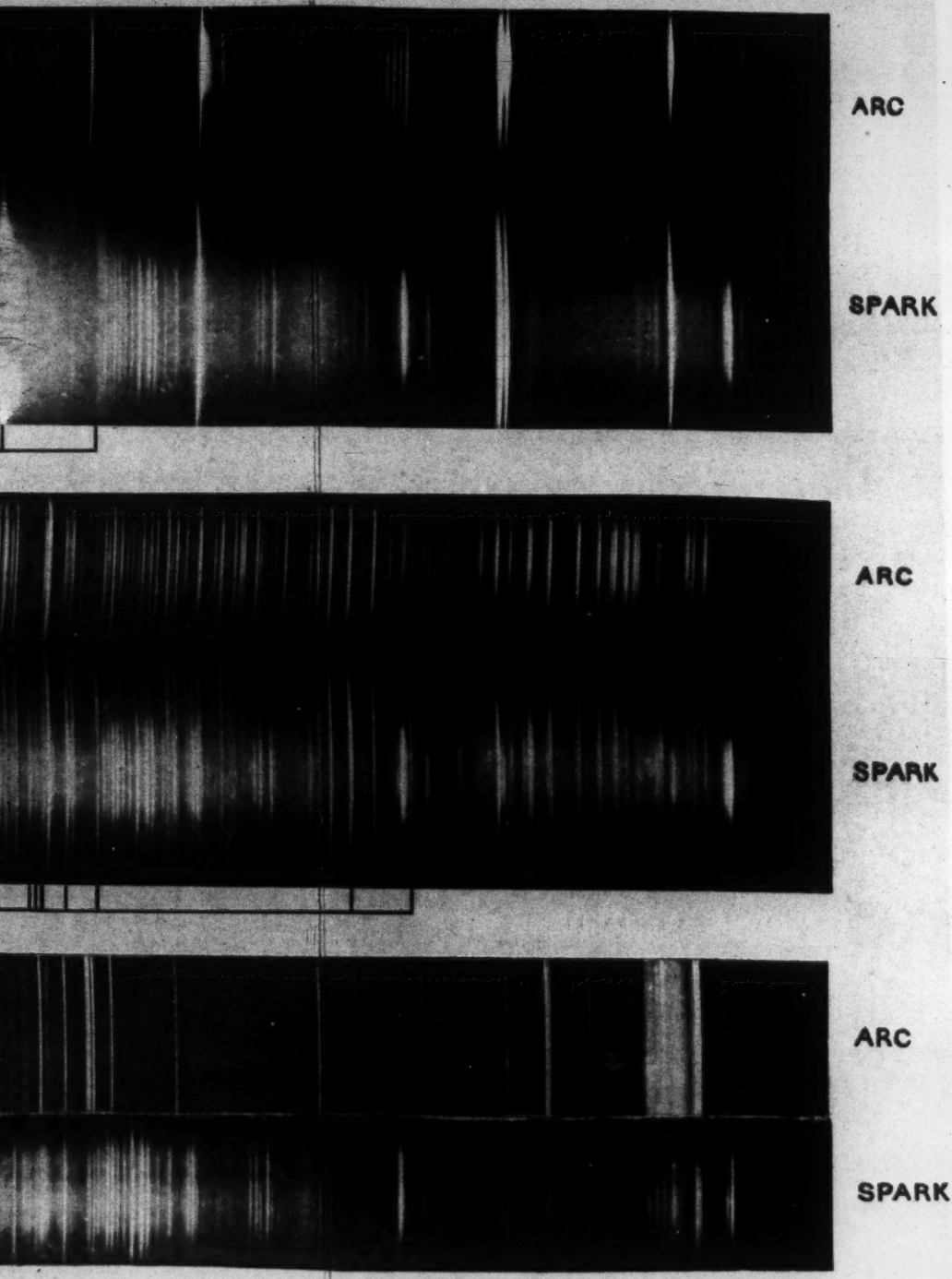
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SPECTRA OF Mo, Fe, & Ca, SHEWING ENHANCE

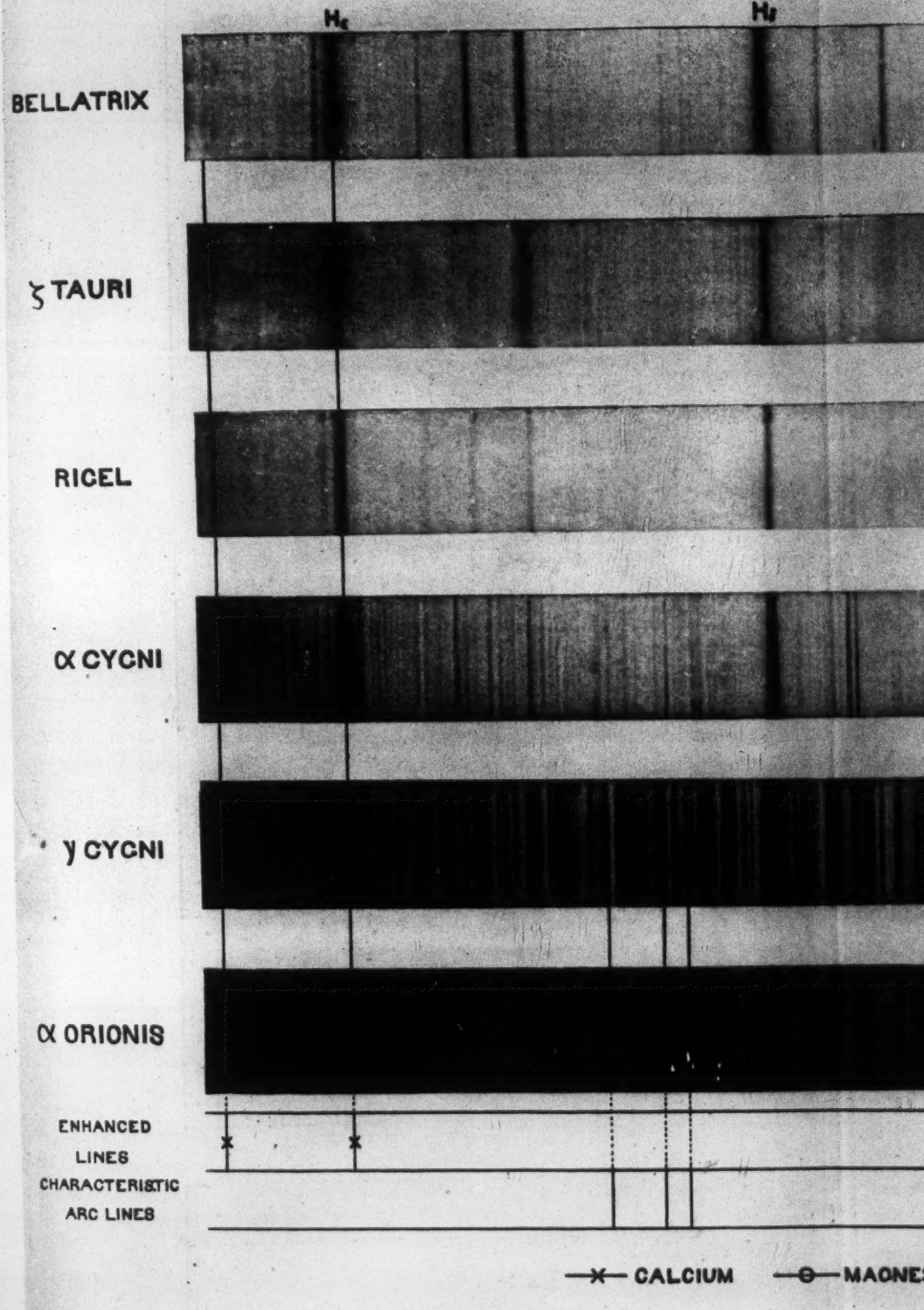


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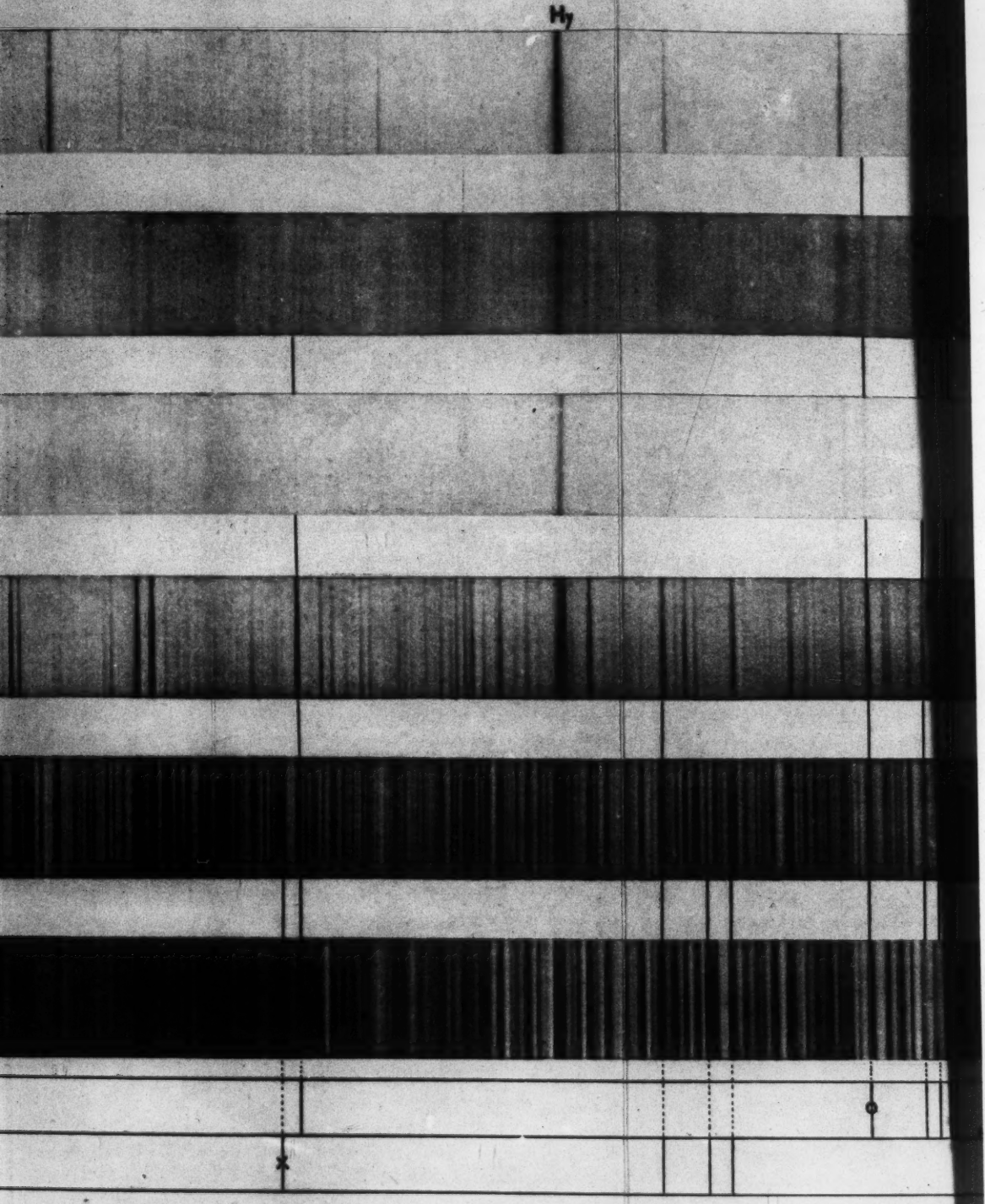


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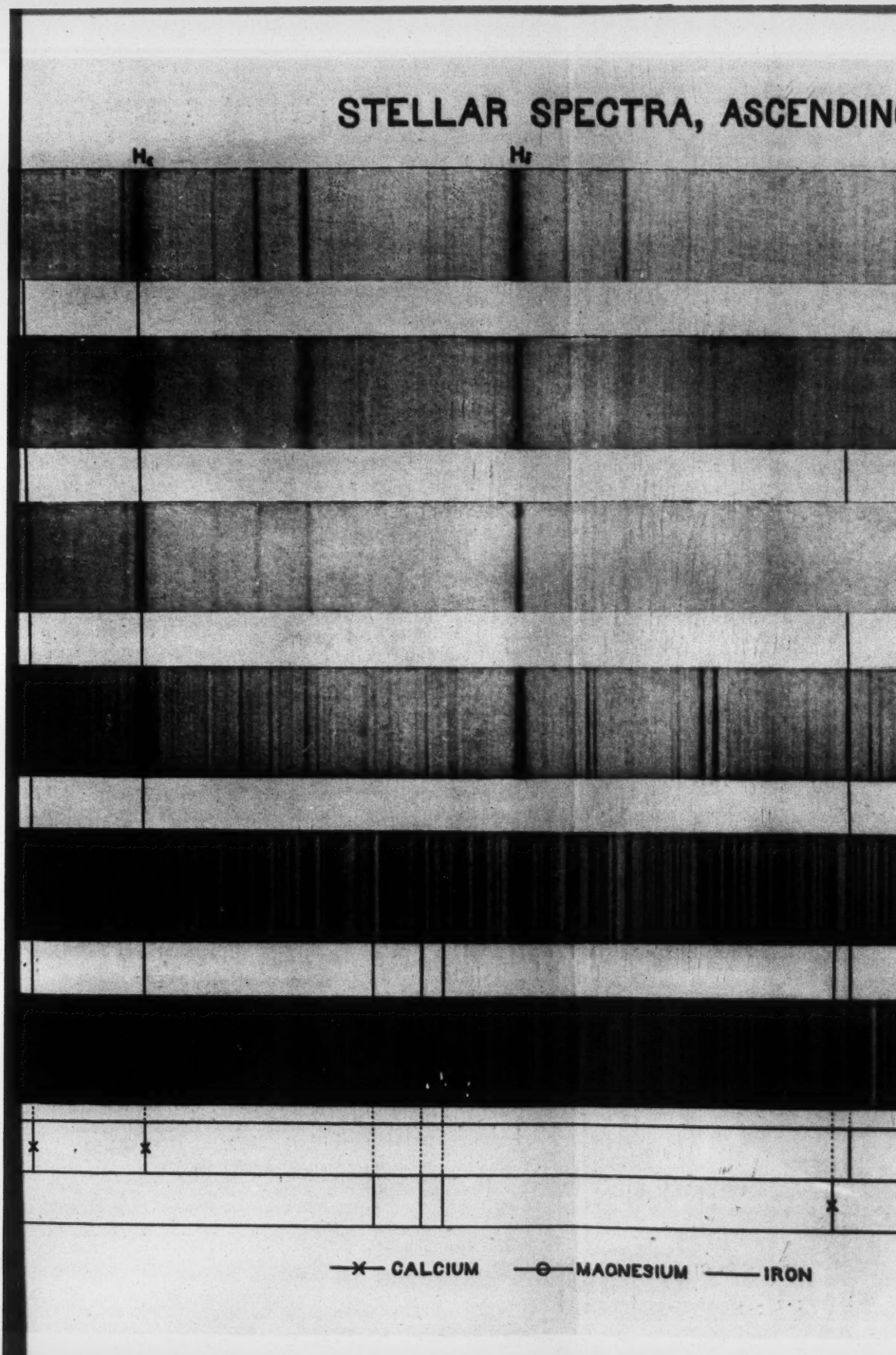
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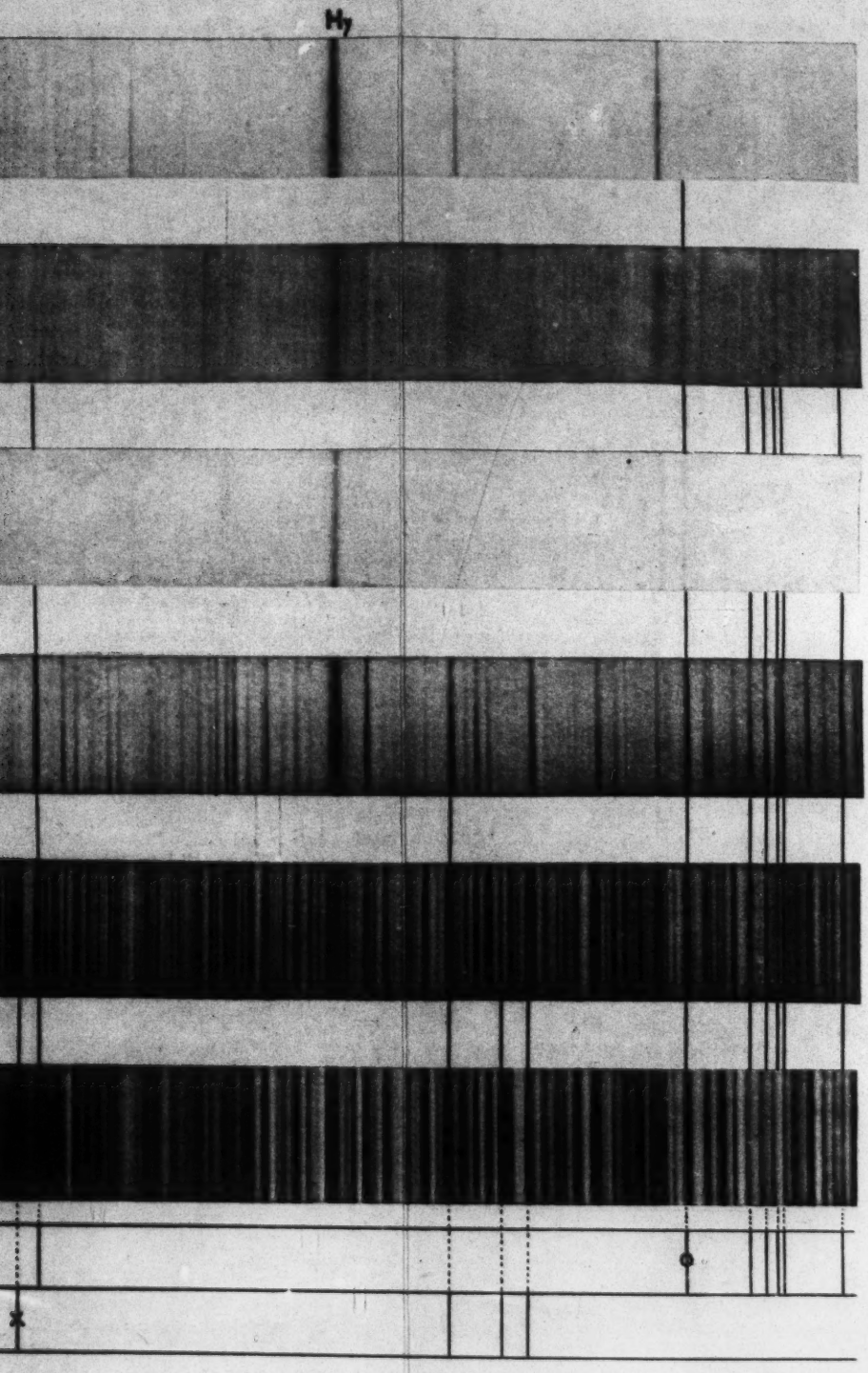
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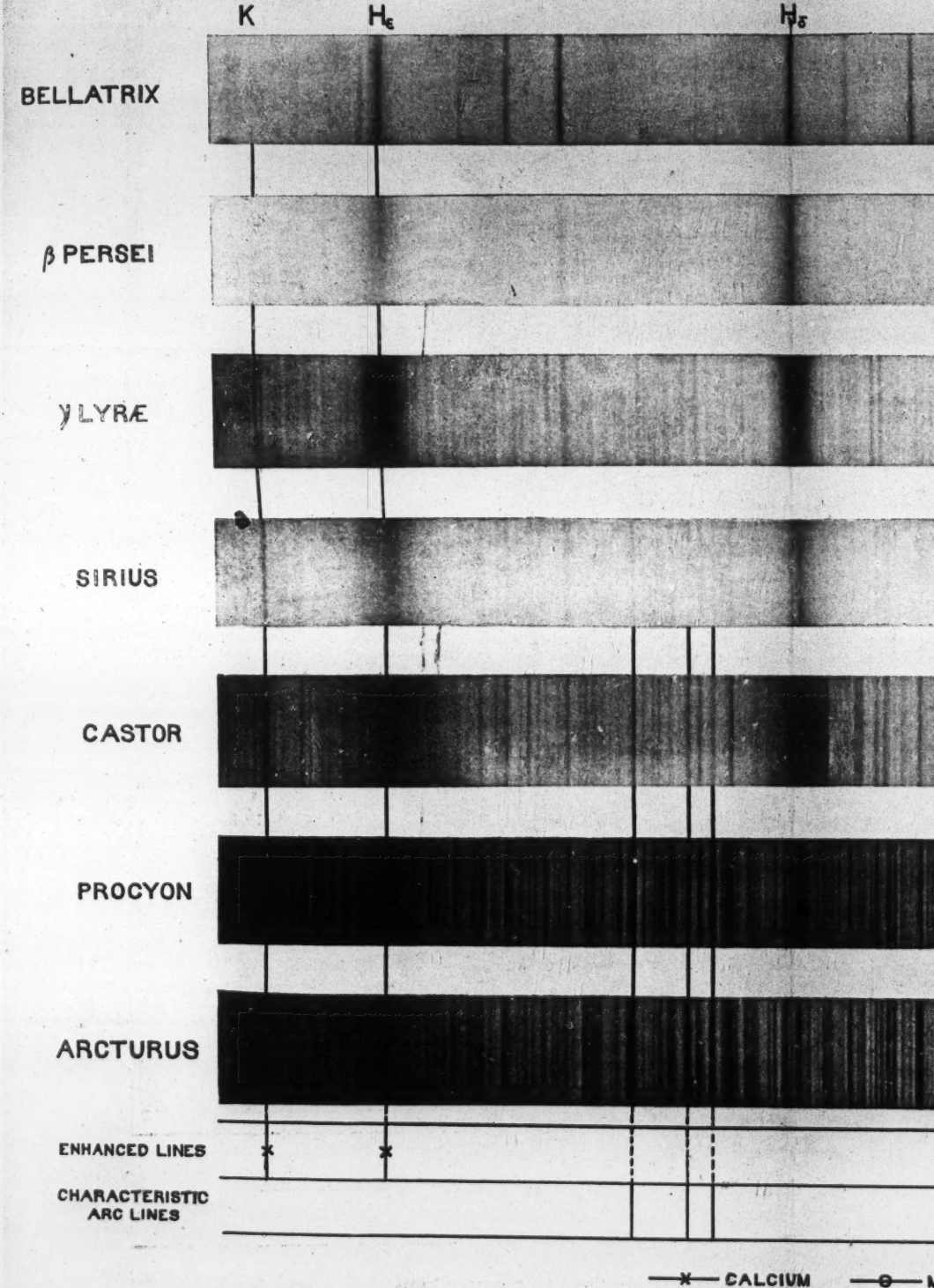

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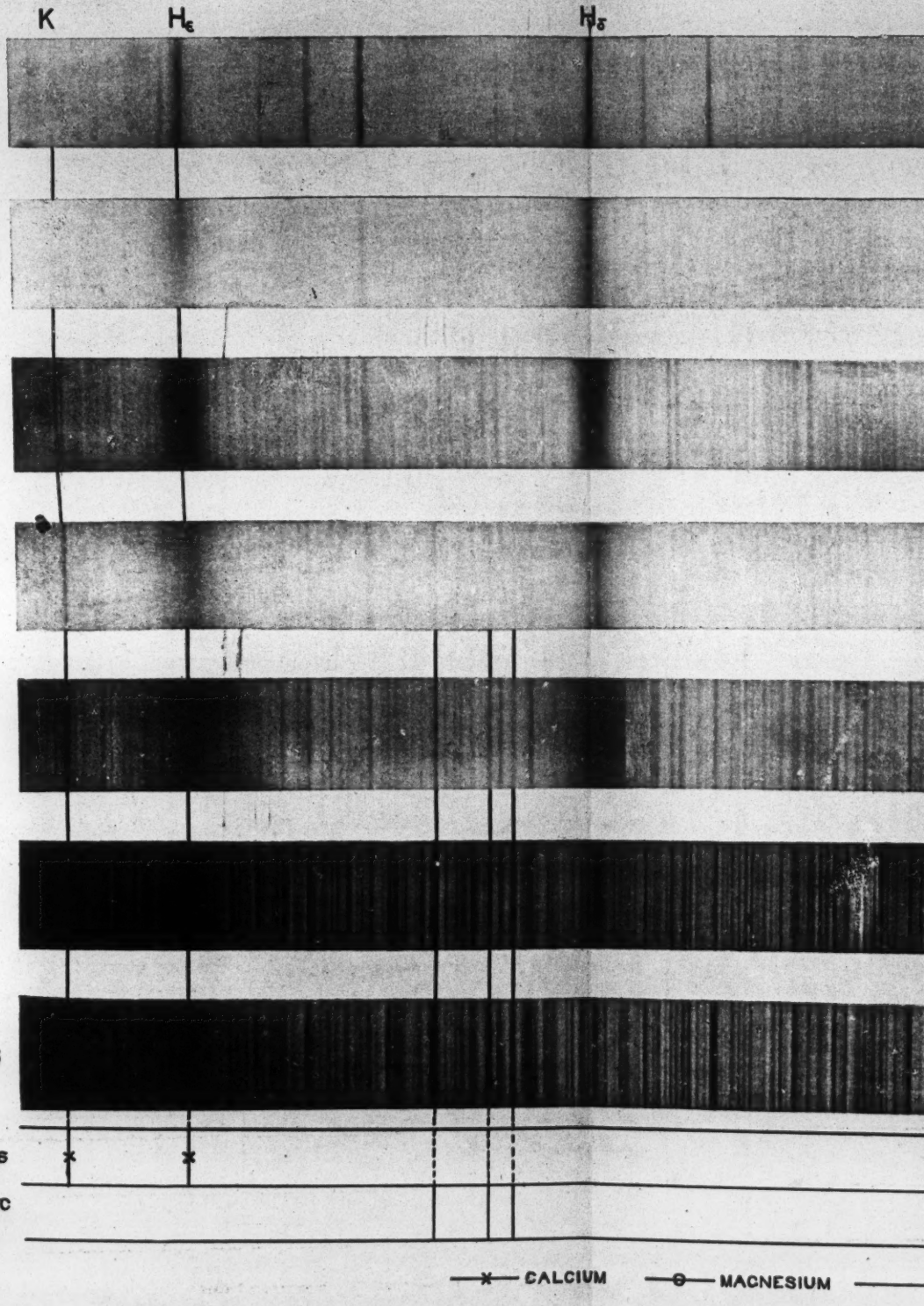


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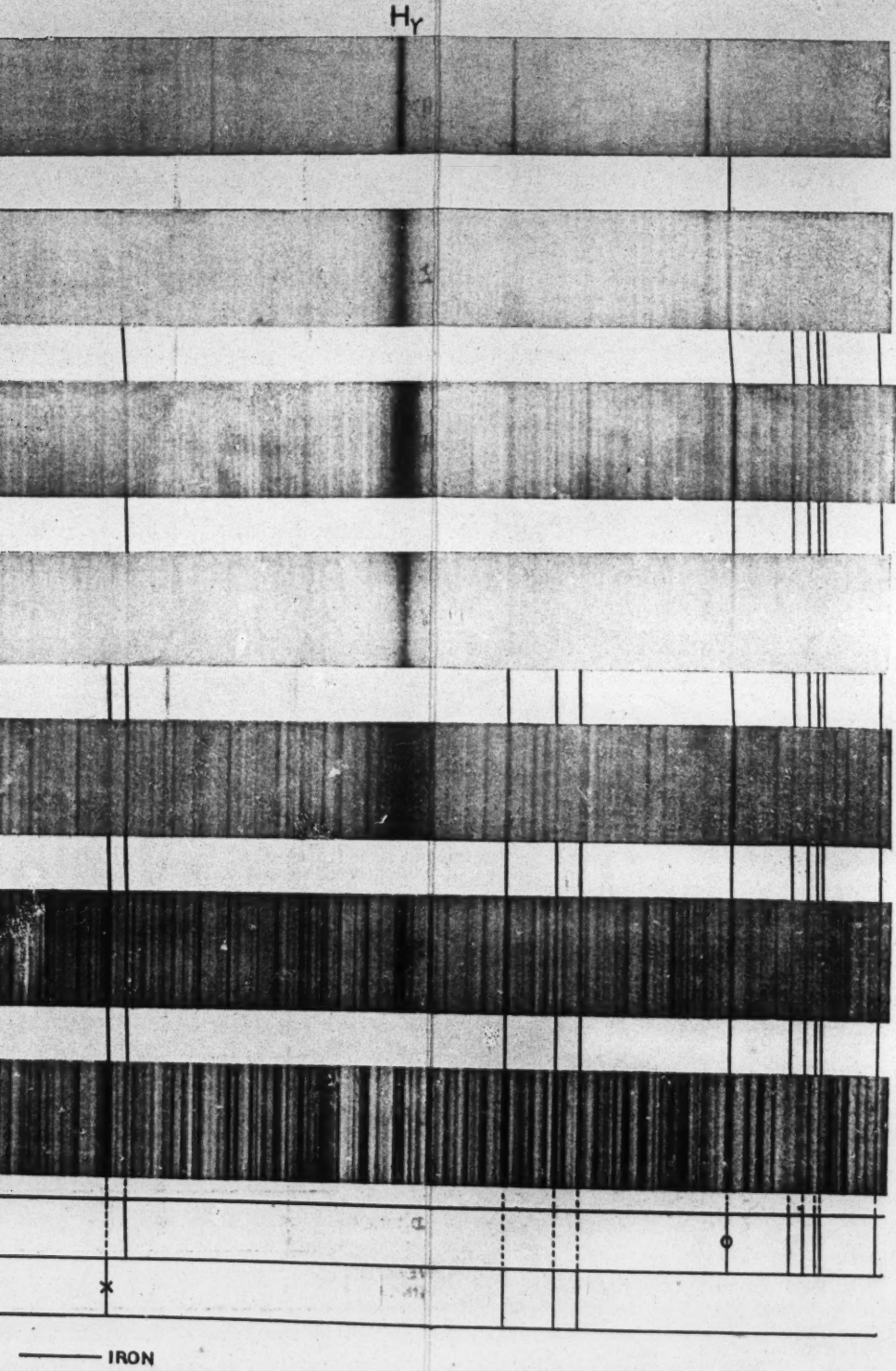


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different lengths, and I indicated by means of photographs how very definite these phenomena were. The adoption of this method of work enabled me to establish that when a metallic vapour is subjected at any one temperature to admixture with another gas or vapour, or to reduced pressure, its spectrum becomes simplified by the abstraction of the shortest lines and by the thinning of many lines.

In the same paper, I pointed out that "these observations have an important bearing upon the solar spectrum, for the reason that, as is well known, all the lines known to exist in the spectrum of an element supposed to be present in the sun's atmosphere are not in all cases reversed" (p. 261). Maps were given which showed "that *invariably the reversed lines are simply those which are longest in the spectrum.* . . . It supplies us at once with the true test to apply to the reversal of solar lines and a guide of the highest value in spectrum observations of the chromosphere and photosphere" (p. 264).

In another communication,* in November, 1873, I added that "the test formerly relied on to decide the presence or absence of a metal in the sun (namely, the presence or absence of the brightest and strongest lines of the metal in question in the average solar spectrum) was not a final one; and that the true test was the presence or absence of the longest line, being that which remains longest in the spectrum when the pressure of the vapour is reduced."

In this paper I gave a photograph of the spectrum of iron produced when an image of a horizontal *arc* was projected on to the vertical slit of a Steinheil spectroscope. A spectrum of long and short lines is obtained in this way, precisely as in the case of the spark, except that the phenomena are much better seen. The long lines represent the vapours which extend furthest from the centre of the arc, the short lines those which exist only at the centre.

On the strength of the criterion thus established, I was enabled to announce the presence of many metallic elements in the sun's atmosphere.

Some Short Lines indicate the Effects of High Temperatures.

It was generally assumed in the first instance that the short line were true products of the greater heat of the core of the arc.

Subsequent work with the jar spark, in 1876† and 1878,‡ showed that at spark temperatures some of the shorter arc lines behaved

* 'Phil. Trans.,' vol. 164, Part 2, p. 490.

† 'Roy. Soc. Proc.,' vol. 24, p. 352.

‡ *Ibid.*, vol. 28, p. 157.

differently from others. Among these lines may be mentioned the two lines of calcium producing the solar lines H and K and two lines of iron at 4924·1 and 5018·6 (λλR). These lines were enhanced in intensity on passing to the temperature of the spark, while a large number of similar lines disappeared, and it was found that the lines thus enhanced in intensity were of considerable importance in the spectrum of the solar chromosphere.

These facts seemed to show that short lines might be produced by two causes: (1) the increased temperature of the core; (2) the rapid breaking up of the solid metal used as poles into various complex molecular groupings as the vapours passed from the core to the outer edge of the arc.* This last action was apparently responsible for by far the greater portion of the short lines.

Flame, Arc, and Spark Lines.

In 1879 I attempted to carry the matter further by volatilising those substances which give us spectra in a Bunsen flame and passing a strong spark through the flame, first during the process of volatilisation, the substance being put into the flame just below the platinums, and then after the temperature of the flame has produced all the simplification it is capable of producing,* the substance in this case being introduced into the base of the flame. The passage from flame to spark represented a stronger case than the passage from arc to spark, and the view that the above-named two causes were at work was greatly strengthened by the observation of magnesium, lithium, and other metals recorded. In the first place, the differences observed in dealing with different quantities were attributed to the fact that "the more there is to dissociate, the more time is required to run through the series, and the better the first stages are seen."

Further, lines invisible in the flame spectrum when the spark was not passing were rendered visible by the passage of the spark. The blue line of lithium about λ 4602 and the line of magnesium about λ 4481 may be given as examples. Some of the flame lines were dimmed or became invisible at the time of the production of the new lines. The lines intensified by the spark in the flame were the same as those enhanced on the passage from the arc to the spark.

This strengthened the view that the result of a higher temperature was to produce an important change in the spectrum, and it was conceivable that in a space entirely heated up to the highest temperature the spectrum would consist entirely of the enhanced lines.

The employment of the flame in the experiment just referred to suggested a series of observations at flame temperatures with a view

* 'Roy. Soc., Proc.,' 1879, vol. 30, p. 22.

of determining exactly the difference between the flame and arc spectra.

The general result of this line of work was to show that there was a step similar in kind to that from arc to spark between the flame and arc. To take iron as a case in point, a few lines only constitute the spectrum of the flame. The number is enormously increased on passing to the arc, but none of the flame lines are dropped.

A very important fact to be borne in mind is that the intensities of the lines common to the flame, arc, and spark differ greatly at the three temperatures, so that the three stages of flame, arc, and spark lines are generally recognised.

II. Application of the Early Results to the Investigation of Stellar Spectra.

General Remarks.

In attempting to assign origins to the lines seen in the spectrum of the sun or stars, it is clear, from what has been stated, that we shall not get a perfect matching with terrestrial lines, either in regard to the intensities or lengths of lines, unless the terrestrial lines in question are those seen at a temperature closely approximating to that of the particular part of the sun or star in which the vapour which produces the absorption exists.

In the atmosphere of the stars we have probably the closest approximation open to our observation of that equally heated space condition to which I have previously referred.

The question arises whether we have any means, independently of the line phenomena we are now discussing, of determining whether the effective absorbing region of the stellar atmospheres are at the same temperature or whether they vary. And in the latter case, can we go further and arrange them approximately in the order of temperature?

First Indications of Relative Stellar Temperatures.

On this point I wrote as follows in 1892*: "An erroneous idea with regard to the indications of the temperature of the stars has been held by those who have not considered the matter specially. It has been imagined that the presence of the series of hydrogen lines in the ultra-violet was of itself sufficient evidence of a very high temperature. The experiments of Cornu,† however, have shown that the complete series of lines can be seen with an ordinary spark without jar. Hence the high temperature of such a star as Sirius is not

* 'Phil. Trans.,' 1893, vol. 184, p. 721.

† 'Journ. de Physique,' 1886, series 2, vol. 5, p. 341.

indicated by the fact that its spectrum shows the whole series of hydrogen lines, *but by the fact that there is bright continuous radiation far in the ultra-violet.*"

We shall not go far wrong in supposing that the star with the most intense continuous radiation in the ultra-violet is the hottest, independently of absorbing conditions, which, in the absence of evidence to the contrary, we must assume to follow the same law in all.

An inquiry into the facts placed at our disposal by the stellar photographs taken at Kensington, shows that there is a considerable variation in the distance to which the radiation extends in the ultra-violet, and that the stars can be arranged in order of temperature on this basis.

Judged by this criterion alone, some of the hottest stars so far observed are γ Orionis, ζ Orionis, α Virginis, γ Pegasi, η Ursæ Majoris, and λ Tauri. Of stars of lower, but not much lower, temperature than the above, may be named Rigel, ζ Tauri, α Andromedæ, β Persei, α Pegasi, and β Tauri.

Comparison of Terrestrial and Stellar Line Spectra.

Having, then, this independent determination of relative temperature, we are in a position to study the results of the comparison of terrestrial and celestial line spectra.

As the variations of spectra with temperature are very striking in the case of iron, calcium, and magnesium, the spectra of these substances afford a ready means of checking the relative temperature of the absorbing region of stars as determined by a consideration of the extension of the continuous radiation into the violet and ultra-violet.

But, before the results of such inquiries can be properly used, some general considerations have to be passed under review.

The Loci of Absorption in Stellar Atmospheres.

Assuming that the most valid absorbing vapours in any particular star are all near one temperature, we can proceed to investigate the origins of the spectrum lines by first getting a clue as to the probable temperature from the extent of continuous spectrum, and then inquiring into the presence or absence of the lines which are longest in the spectra of various substances at that temperature. If, however, the absorptions take place at different levels in the atmosphere of a star, the proper spectrum of each substance to be thus investigated can only be determined by a comparison of the stellar with the terrestrial lines of the substance under varying temperature conditions.

This method of looking for the longest lines will fail in the case of stars which are hotter than our hottest spark. In such case, therefore, we must necessarily rely on a comparison with lines which, from our study of the spectra at different temperatures, would most probably be longest in the spectrum at a temperature higher than any at which experiments can be carried on.

It is in connection with such an inquiry as this that the study of the conditions of the sun's atmosphere is of supreme importance. It is obvious that a knowledge of them must be of the utmost value in enabling us to apply a well-established series of facts, gathered in the case of the star nearest to us, to the phenomena presented by the more distant bodies.

Observations of eclipses, especially those of 1882 and 1893, provide us with a series of results with which to approach the question of the absorption phenomena presented by the stars, and we have seen that the whole question of the classification of stars depends absolutely upon their absorption phenomena.

Now in many of these bodies the atmosphere may be millions of miles high. In each star the chemical substances in the hottest and coolest portion may be vastly different; the region, therefore, in which the absorption takes place, which, spectroscopically, enables us to discriminate star from star, must be accurately known before we can obtain the greatest amount of information from our inquiries.

The loci of the various absorptions in a mass of gas such as a star of the highest temperature, have not yet been sufficiently studied.

I may say that for some time I was of opinion that in the sun many of the darkest lines indicated absorptions high up in the atmosphere, for the reason that the bright continuous spectrum of the lower levels might have an important effect upon line absorption phenomena by superposing radiation, and so diminishing the initial absorption. The observations of the eclipse of 1893, however, indicate that this opinion is probably only strictly true when the strata of the sun's atmosphere close above the photosphere are considered.

If we are justified in arguing from a star with a photosphere as well developed as that of the sun, to one in which it is in all probability much less marked, in consequence of a much higher temperature, then we must consider that the absorptions which mark out the various star groups are more conditioned by the temperatures of the hottest regions merely than by the thickness of the absorbing atmospheres, or by the densities of the various vapours. Another consideration to be borne in mind is that if the atmospheres are in part composed of condensable vapours, and not entirely of gases permanent at all stellar temperatures, condensation must always be going on outside at the region of lowest temperature.

At this point it is important to consider the conditions of that part

of the sun's atmosphere where it is known beyond all question that certain, but not all, of the absorptions which produce the Fraunhofer lines take place.

In my paper on the eclipse of 1893,* I referred at length to this point. The matter is so important that I do not hesitate to quote what I then said in the present connection:

"As a result of solar spectroscopic observations, combined with laboratory work, Dr. Frankland and myself came to the conclusion, in 1869, that at least in one particular, Kirchhoff's theory of the solar constitution required modification. In that year we wrote as follows:—†

"'May not these facts indicate that the absorption to which the reversal of the spectrum and the Fraunhofer lines are due takes place in the photosphere itself, or extremely near to it, instead of in an extensive outer absorbing atmosphere?'

"In an early observation of a prominence on April 17th, 1870, I found hundreds of the Fraunhofer lines bright at the base, and remarked that a 'more convincing proof of the theory of the solar constitution put forward by Dr. Frankland and myself could scarcely have been furnished.'‡

"During the eclipse of 1870, at the moment of disappearance of the sun, a similar reversal of lines was noticed; we had, to quote Professor Young, 'a sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality.' On these observations was based the view that there was a region some 2″ high above the photosphere, which reversed for us *all* the lines visible in the solar spectrum; and on this ground the name 'reversing layer' was given to it.

"Continued observations, however, led me, in 1873, to abandon the view that the absorption phenomena of the solar spectrum are produced by any such thin stratum, and convinced me that the absorption took place at various levels above the photosphere. I need not give the evidence here; it is set forth in my 'Chemistry of the Sun.'§ On the latter hypothesis the different vapours exist normally at different distances above the photosphere, according to their powers of resisting the dissociating effects of heat.‖

"My observations during the eclipse of 1882, in the seven minutes preceding totality, to my mind set the matter at rest. 'We begin with one short and brilliant line constantly seen in prominences, never seen in spots. Next another line appears, also constantly seen in

* 'Phil. Trans.,' 1896, vol. 187, A, p. 603.
† 'Roy. Soc. Proc.,' vol. 17, p. 88.
‡ *Ibid.*, vol. 18, p. 358.
Chapter XXII, pp. 303–309.
'Roy. Soc. Proc.,' vol. 34, p. 292.

prominences; and now, for the first time, a *longer* and thinner line appears, occasionally noted as widened in spots; while, last of all, we get, very long, very delicate relatively, two lines constantly seen widened in spots, and another line, not seen in the spark, and never yet recorded as widened in spots.'*

"Similar observations in the same part of the spectrum were made by Professor Turner in 1886.† His observations were made under less favourable conditions than those in Egypt, and in the absence of statements as to the relative lengths of the lines observed, it is impossible to utilise them.

"This is one of the most important points in solar physics, but there is not yet a concensus of opinion upon it. Professor Young and others, apparently, still hold to the view first announced by Dr. Frankland and myself in the infancy of the observations, that the Fraunhofer absorption takes place in a thin stratum, lying close to the photosphere."

I next proceeded to discuss the numerous photographs obtained during the eclipse, and I gave a map showing that there was only the slightest relation between the intensities of the lines common to the Fraunhofer and the eclipse spectrum, and further, that only a few of the Fraunhofer lines are represented at all. Not only this, but in the eclipse photographs there are many bright lines not represented at all among the Fraunhofer lines.

The chromosphere, then, is certainly not the origin of the Fraunhofer lines, either as regards intensity or number. From the eye observations made since 1868, there is ample evidence that the quiescent chromosphere spectrum indicates a higher temperature than that at which much of the most valid absorption takes place; in other words, that the majority of the lines associated with lower temperature are produced above the level of the chromosphere.

The eclipse photographs, however, at the same time afford evidence by the relative lengths of some of the lower temperature lines that we need not locate the region which produces them at any great height above the chromosphere.

The solar evidence, then, is that most of the line absorption is produced in, and not very far above, the chromosphere. This is a conclusion we are bound to accept in a discussion of the origin of stellar absorption in the absence of evidence to the contrary.

We have no right to assume that the absorption will be produced at the top of the atmosphere in one star, and in the bottom in another, when the atmospheres are once relatively quiescent.

* 'Roy. Soc. Proc.,' vol. 34, p. 297.

† 'Phil. Trans.,' 1889, vol. 180, A, p. 391.

III. Recent Photographic Investigations on the Spectra of Metals at High Temperatures.

At the time at which the earlier inquiries to which I have already referred were made, it was only possible for the most part to deal with eye observations of the heavenly bodies. The results were, therefore, limited to the visible spectrum.

During the last few years I have been fortunate enough to obtain photographs of the brighter stars and of the sun's chromosphere during eclipses; it became of importance, therefore, to extend the observations of terrestrial spectra into the photographic regions for the purpose of making the comparisons which were necessary for continuing the inquiry.

Iron.

The facts with regard to changes in the spectrum of iron were shown by my early experiments to be not much less striking than those noted in the case of calcium, magnesium, and other metals.

For the last twenty years I have devoted special attention to iron, and noted many stellar and terrestrial coincidences. I shall begin my account of the new work by dealing with that metal.

Among the iron lines are two triplets, or sets of three lines, giving an example of repetitions of structure in differents parts of the spectrum; one of them is less refrangible than G, and the other falls between *h* and H. In 1878 I wrote as follows:—

"In many photographs in which iron has been compared with other bodies, and in others, again, in which iron has been photographed as existing in different degrees of impurity in other bodies, these triplets have been seen almost alone, and the relative intensity of them as compared with the few remaining lines, is greatly changed. In this these photographs resemble one I took three years ago, in which a large coil and jar were employed instead of the arc. In this the triplet near G is very marked; the two adjacent lines more refrangible near it, which are seen nearly as strong as the triplet itself in some of the arc photographs I possess, are only very faintly visible, while dimmer still are seen the lines of the triplet between H and *h*."*

It was also pointed out at the same time that the lines of iron appearing in the chromosphere did not make their appearance in the order of their lengths in the arc.

For the new inquiries I have employed two storage cells giving a current of 7 amperes at 8 volts, with an Apps Intensity Coil giving a spark of 10 inches and a jar capacity of between 40 and 50 gallons.

To clear the ground, it was important to determine whether the

* 'Roy. Soc. Proc.,' vol. 28, p. 172.

generally observed dropping-out of lines in the spark depends upon the diminished quantity of incandescent vapour as compared with that in the arc.

With the brilliant spark obtained under the new conditions there is little difference betweeen arc and spark with regard to the number of lines. Hence it may be concluded that the small number of lines previously recorded in the spark spectrum was not an effect of increased temperature, but one due to the small quantity of vapour produced by the use of a small coil.

The next point was to inquire if the photographic region of the spectrum reveals lines which are the equivalents of those at 4924·1 and 5018·6 (Rowland's scale), which I had previously shown to be enhanced in passing from the arc to the spark spectrum.*

Seven additional lines were detected in the photographs at the following wave-lengths on Rowland's scale.

4233·3
4508·5
4515·4
4520·4
4522·7
4549·6
4584·0.

These have been confirmed by a reference to the map of the spark spectrum of iron published by Dr. McClean,† and attention was drawn to those at 4584·0 and 4233·3 in my paper on the arc spectrum of electrolytic iron.‡

All these appear as short lines in the arc spectrum, so that the view that the short lines which appear in the arc spectrum can be divided into two categories, one including the lines which are brightened in the spark, and the other the lines which are not so enhanced, is confirmed.

Having thus established that there are differences between the arc and spark spectrum, and that these differences are not due to the different quantities of vapour in the two cases, it must be concluded that a difference of temperature is the main cause of change.

Including the flame spectrum then, four distinct temperature stages are indicated by the varying spectrum of iron:—

(1) The flame spectrum, consisting of a few lines only, including the well-known triplets and many strong lines in the ultra-violet.

(2) The arc spectrum consisting, according to Rowland, of 2000 lines or more.

* 'Roy. Soc. Proc.,' vol. 32, p. 204.
† 'Monthly Notices R.A.S.,' vol. 52.
‡ 'Phil. Trans.,' vol. 185, A, pp. 995, 996.

(3) The spark spectrum, differing from the arc spectrum in the enhancement of some of the short lines and the reduced brightness of others.

(4) A spectrum consisting of the lines which are intensified in the spark. This we can conceive to be visible alone at the highest temperature in a space efficiently shielded from the action of all lower ones, since the enhanced lines behave like those of a metal when a compound of a metal is broken up by the action of heat.

A complete list of the iron lines seen at the different temperatures would be too long to reproduce here, so that the following statement of intensities is limited to the lines enhanced in the spark. The behaviour of these lines under the different conditions of experiment is as follows :—

Lines of Iron which are enhanced in Spark.

Wave length. (Rowland).	Intensity in flame.	Intensity in arc (K & R) Max. = 10.	Length in arc (L) Max. = 10.	Intensity in spark (T). Max. = 10.	Intensity in hot spark (L). Max. = 10.
4233·3	—	1	—	—	4
4508·5	—	1	—	—	4
4515·4	—	1	—	—	4
4520·4	—	1	—	—	2
4522·7	—	1	3	—	4
4549·6	—	4	5	—	6
4584·0	—	2	4	—	7
4924·1	—	1	3	6	6
5018·6	—	4	—	—	6

K & R = Kayser and Runge, T = Thalén, L = Lockyer.

The arc and spark spectra are photographically compared in Plate 1. The photographs were taken with an instrument having two prisms of 60°, a collimator of 3-in. aperture and 5-ft. focus, and a photographic lens of 19 in. focal length. The enhanced lines are indicated at the bottom. It should be remarked that the enhancement of the line at 4233·3 is not clearly seen with this dispersion, but it is quite conspicuous in a photograph recently taken in the first order spectrum of a Rowland grating of 21 ft. 6 in. radius.

Calcium.

I next proceed to consider the results obtained in the case of calcium.

Among the chief observations of the spectrum of this metal are those made by Thalén, Kayser and Runge, and myself.

Thalén chiefly confined himself to a study of the spectrum at the

temperature of the spark, the observations of Messrs. Kayser and Runge have been limited to the arc spectrum, while my own investigations have included all conditions of temperature available in laboratory experiments.

As I showed in 1876,* the most characteristic low temperature line is that at λ 4226·9, while the H and K lines are pre-eminent at high temperatues. The new work with the spark from the large intensity coil and large jars has shown that all the lines recorded by Kayser and Runge in the arc spectrum appear also in the spark spectrum, but with the exception of H and K, and two lines at wave-lengths 3706·18 and 3737·08, which do not appear to have been previously recorded in the spark, they appear with reduced relative intensities.

These two ultra-violet lines are enormously enhanced in the spark, as will be seen on reference to the comparison photograph of the arc and spark spectra given in Plate 1.

As in the case of iron, four temperature steps can be recognised.

(1) The flame spectrum, in which the blue line 4226·9 is predominant, H and K, and a few other lines being very feeble.

(2) The arc spectrum, in which the H and K lines are of about the same brightness as the blue line, while other feebler lines also appear.

(3) The spark spectrum, in which nearly all the lines of the arc spectrum are seen, but with reduced intensities, except in the case of H and K, which remain very bright, and two lines at 3706·18 and 3737·08, which are also very bright.

(4) A spectrum consisting of the two lines at 3706·18 and 3737·08 and the H and K lines, corresponding to a temperature higher than the average temperature of the spark, as before explained.

The complete spectra actually recorded are shown in the following table (p. 162).

Magnesium.

Among other substances investigated in my earlier work was magnesium.

I showed in 1879† that in the flame spectrum the two less refrangible members of the *b* group were seen associated with a less refrangible line at 5210, making a triplet with them, while a line in the blue at wave-length 4571·3, and a series, of flutings were also seen; on passing the spark, the blue line of the flame disappears, as well as the flame companion to *b*, while two new blue lines make their appearance at wave-lengths 4481·3 and 4703·3.

Among the spark lines is one at λ 4481, of which I had previously

* 'Roy. Soc. Proc.,' vol. 24, p. 352.

† *Ibid.*, vol. 30, p. 22.

Calcium.

Wave-length. (K and R.)	Intensity in flame (L). Max. = 10.	Intensity in arc (K & R). Max. = 10.	Length in arc (L). Max. = 10.	Intensity in spark (T). Max. = 10.	Intensity in hot spark (L). Max. = 10.
3706·18	—	4	—	—	8
3737·08	—	4	—	—	10
K 3933·83	3	10	10	10	10
3949·09	—	4	—	—	1
3957·23	—	6	2	—	1
H 3968·63	3	10	10	10	10
3973·89	—	6	4	—	2
4092·83	—	2	2	2	1
4095·25	—	2	—	2	trace
4098·82	—	4	4	2	1
4226·91	10	10	10	10	5
4238·00	—	—	2	—	—
4240·58	—	4	—	2	—
4283·16	—	8	6	8	2
4289·51	—	8	6	6	2
4299·14	—	6	6	6	2
4302·68	1	10	6	10	3
4307·91	—	8	6	6	1
4318·80	1	8	6	8	2
4355·41	—	6	4	—	1
4425·61	—	10	6	10	2
4435·13	1	10	8	10	3
4435·86	—	8		2	
4454·97	2	10	8	10	4
4456·08	—	8		2	
4456·81	—	4		—	
4508·04	—	1	Out of range.	—	1
4509·89	—	1		—	
4512·73	—	1		—	1
4527·17	—	6		—	1
4578·82	—	8		4	1
4581·66	—	8		4	1
4586·12	—	10		4	2
4685·40	—	4		—	—

K & R = Kayser and Runge, T = Thalén, L = Lockyer.

written as follows in November 1872:* "This is a very brillant winged line, but it appears short. Thalén makes it of the same intensity as the two at 4703·5 and 4586·5; but while this is excessively bright to me, 4703·5 is faint and 4586·5 invisible." Taking *b* as having a length denoted by 4, I gave the length of this line at 4481 as 1; I also stated that it was not seen in the spectrum of the chloride, although the *b* group was distinctly seen. The line at 4481 has not to my knowlege been recorded by any observer as present in the arc spectrum, but a recent photograph shows it as a rather feeble line in the arc between poles consisting of magnesium.

* 'Phil. Trans.,' 1873, vol. 163, Part 1, p. 267.

Using the large coil and jars, the line has also been photographed in the spectrum of the chloride; indeed all the lines recorded in the arc by Kayser and Runge have been photographed in the spark spectrum of the metal.

The work with the large jars has also resulted in the detection of another line of magnesium about wave-length 4395 which does not appear in the arc spectrum, and the line about 4587·4 observed by Thalén also shows itself feebly. The former of these is fairly bright and seems to be closely associated with 4481.

Again there are four distinct temperature steps, namely:

(1) The flame spectrum, represented by lines at 4571·3 and *b*, a triplet in the ultra-violet commencing with a line at 3734, and two flutings, one commencing at 5210 and the other at 5006·5.*

(2) The arc spectrum, comprising *b*, a line at 4352·18, and another triplet in the ultra-violet commencing with 3838·4, 4481 being almost invisible, while 4395 and 4587 are quite invisible.

(3) The spark spectrum, including all the arc lines, but with 4481 intensely bright, 4395 fairly bright though short and 4587·4 rather feeble.

(4) As 4481, 4395, and 4587·4 are much intensified in the spark spectrum, we can conceive a fourth stage at a still higher temperature when magnesium would be represented by these lines alone. The complete spectra of magnesium under the three conditions at which observations can be made are indicated in the accompanying table (p. 164).

The arc and spark spectra, as photographed with the moderate dispersion to which reference has already been made, are shown in Plate 1, the three enhanced lines being indicated as in the case of iron and calcium.

IV. Application of the recent Investigations to Stellar Spectra.

In order to study the presence or absence of the enhanced lines in the spectra of the stars, the Kensington series of stellar spectrographs, in which the dispersion is not greatly different from that of the metallic spectra, has been employed.

Although this discussion has specially dealt with the hottest stars, it is necessary to include references to the cooler ones in order to contrast the behaviour of the high temperature lines with those which are characteristic of low temperatures, and further to compare the appearances of the lines at different stellar temperatures.

α Orionis is taken as a typical case of a relatively cool star which

* Messrs. Liveing and Dewar have ascribed these flutings to compounds of magnesium with hydrogen and oxygen respectively, but whether they are due to compounds or to the metal itself is immaterial for my present purpose.

Magnesium.

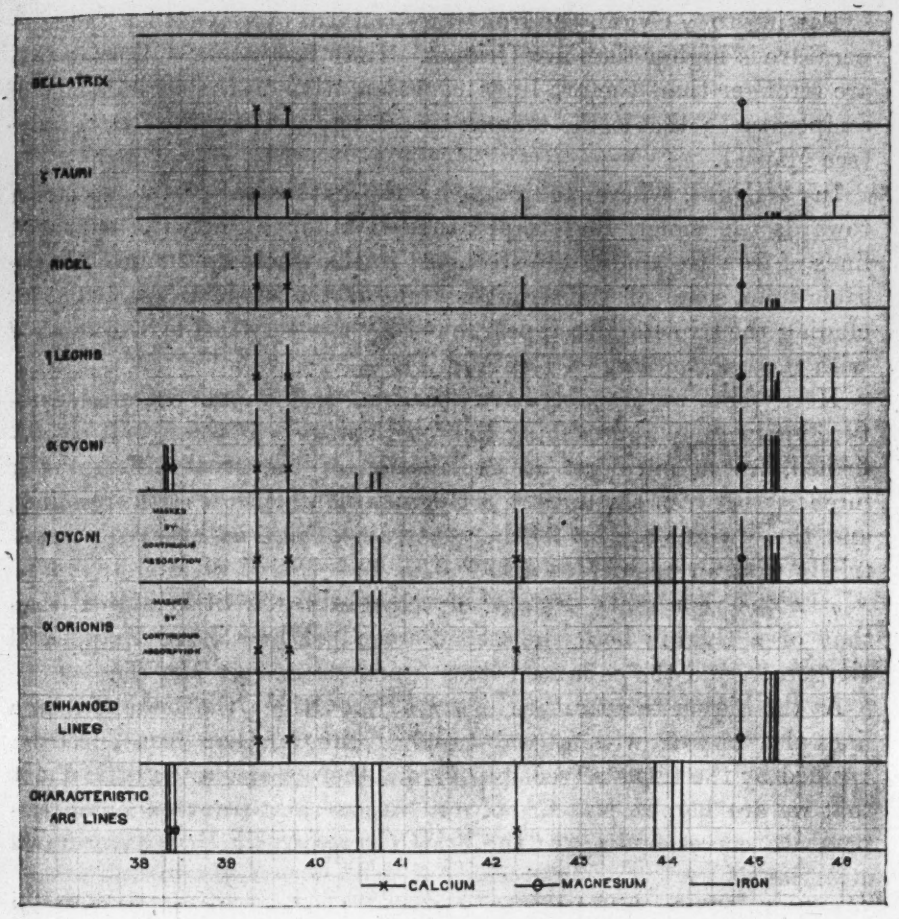
Wave-length. (Rowland's scale.)	Intensity in flame (L). Max. = 10.	Intensity in arc (K & R). Max. = 10.	Length in arc (L). Max. = 10.	Intensity in spark (T). Max. = 10.	Intensity in hot spark (L). Max. = 10.
3720	8	—	—	—	—
3724	8	—	—	—	—
3734	6	—	—	—	—
3829·51	4	10	10	10 (C)	10
3832·46	6	10	10	10 (C)	10
3838·44	8	10	9	10 (C)	10
3850·2	—	4 (L)	—	4	2
3856·2	—	5 (L)	—	4 (H & A)	4
3892·7	—	3 (L)	—	4	4
3896·7	—	3 (L)	—	4 (H & A)	4
3987·08	—	2	—	—	2
4058·45	—	2	—	2 (L & D)	1
4167·81	—	1	—	—	4
4352·18	—	8	10	—	5
4395	—	—	—	—	3
4481·3	—	—	—	8	10
4571·33	7	4	1	2 (L & D)	2
4587·4	—	—	—	4	2
4703·33	—	8	6	8	4
4730·42	—	1	—	—	3
5006·5	10	—	—	—	—
b 5167·55	—	8	3	8	7
b 5172·87	10	10	5	9	9
b 5183·84	10	10	5	10	10
5210	10	—	—	—	—

K & R = Kayser and Runge.
L = Lockyer.
T = Thalén.
C = Cornu.
H & A = Hartley and Adeney.
L & D = Liveing and Dewar.

is bright enough to be studied with sufficient precision for our present purpose, while γ Cygni, α Cygni, η Leonis, Rigel, ζ Tauri, and Bellatrix are taken as types of stars at temperatures which increase in the order named as determined by a comparison of the extension of the spectra into the ultra-violet in accordance with what has already been stated.

The lines photographed in the spectrum of each star shown on the map (Map I) are drawn with lengths proportional to their estimated intensities, the strongest line in each being represented by a line equal to the full width of the strip devoted to each spectrum.

The results shown diagrammatically in Map I are further illustrated by actual photographs of stellar spectra in Plate 2.



MAP I.—Behaviour of metallic lines in stars of increasing temperature.

Iron.

Starting with α Orionis, we find that only two of its more prominent lines may coincide with enhanced lines in the region compared. As there are so many lines in the spectrum of this star, it is possible that these may be only chance coincidences, and this is the more probable since the lines which are most enhanced are certainly absent. Further, I have already shown* that "the temperature of the most important iron-absorbing region in α Orionis is nearer that of the oxy-coal-gas flame than that of the electric arc; it is probable that the average temperature is intermediate between that of the arc and that of the flame, but nearer to the latter."

In α Orionis then we have iron represented by low temperature lines corresponding to a temperature intermediate between the first and second stages.

* 'Phil. Trans.,' 1893, vol. 184, A, p. 703.

Passing to γ Cygni, the iron lines confirm the idea that the temperature is higher than in α Orionis. Here the enhanced lines of iron are stronger than the arc lines representative of the second stage of temperature; that is, the temperature is approaching the fourth stage (see Map I).

In α Cygni, where, to judge by the extension of the spectrum towards the violet, the temperature is much higher, the enhanced lines of iron are among the strongest in the whole spectrum. At the same time, some of the stronger lines of the stage 2 spectrum, including the triplets, also appear, but they are very feeble as compared with the lines of stage 4 (see Map I).

Here, then, we probably have absorbing iron vapour at a temperature very nearly approaching the fourth stage. If this result be confirmed, we at once get an explanation of the great differences of intensity between the lines in α Cygni which coincide with iron lines, and those which appear in the spectrum of iron as observed terrestrially with an unshielded arc.

η Leonis represents a stage of temperature a little higher than that of α Cygni; here the second stage iron lines have disappeared altogether, and the enhanced lines appear alone (see Map I).

At the higher temperature of stars like Rigel, the enhanced iron lines also appear without any trace of the familiar iron spectrum typified by the triplets (see Map I), and there seems to be little doubt that we are here in presence of iron vapour at a transcendental temperature corresponding to the fourth stage to which I have drawn attention.

In ζ Tauri, the iron lines are almost identical with those in Rigel (see Map I).

At the still higher temperature of Bellatrix, all visible traces of the iron spectrum have vanished from the photographs.

General Results with regard to Iron.

The general result of the investigation of the enhanced iron lines in stellar spectra confirms the view that the absorbing regions of the hottest stars exist at a higher temperature than any attainable in laboratory experiments, the spectrum of iron consisting solely of those lines which are enhanced in passing from the arc to the spark. At the same time, some of the lines in the spectra of the hottest stars formerly classed as unknown are now shown to be due to iron.

The fact that even the enhanced lines themselves disappear from the spectra of stars approaching, or at, the acme of stellar temperature raises another question to which reference will be made later.

As I pointed out in a preliminary note on the occurrence of these

lines,* "the enhanced lines may be absent from the spectrum of a star, either on account of too low or too high a temperature. In the case of a low temperature, however, iron is represented among the lines in the spectrum, but at the highest temperature all visible indications of its presence seem to have vanished. This result affords a valuable confirmation of my view that the arc spectrum of the metallic elements is produced by molecules of different complexities, and it also indicates that the temperature of the hottest stars is sufficient to produce simplifications beyond those which have been produced in our laboratories."

The facts which are graphically represented in Map I indicate that so far as these stars are concerned the results, with regard to stellar temperature determined by a study of the iron lines, are identical with those to be gathered from the extension of the radiation spectrum into the violet. Independently of the extensions of spectrum into the violet for different stars, then, the relative temperatures may be determined by a study of the iron lines. Thus, a star, in the spectrum of which iron is represented by traces of the triplets characteristic of the arc spectrum as well as by the enhanced lines, must be cooler, so far as the absorbing iron vapour is concerned, than one in which iron is represented by the enhanced lines alone. Similarly, we must conclude that a star in which iron has no representative lines is hotter than one in which the enhanced lines appear without the arc lines. In practice the iron lines furnish a much more convenient indication of stellar temperature than the continuous spectrum, for the reason that in the case of iron no special photographs are necessary, while for an investigation of the continuous spectrum special photographs of stars with very carefully controlled exposures have to be taken.

Calcium.

The calcium lines in the spectrum of α Orionis indicate that we have in that case a temperature not greatly differing from that of the second stage; the blue line which is characteristic of low temperature, as well as H and K, being very strongly developed (see Map I).

On passing to the stars of successively higher temperatures which have already been studied in the case of iron, it will be seen from Map I, that the blue line has disappeared at the temperature of α Cygni, while the H and K lines persist with gradually reduced intensities up to the hottest stars. The intensities of the H and K lines as compared with the blue line fully bear out the results as to the order of temperature of the stars which has been derived from a

* 'Roy. Soc. Proc.,' vol. 60, p. 475.

comparison of the continuous radiation spectra, and also from the appearances of the iron lines. Unfortunately, the Kensington photographs of stellar spectra which have so far been obtained do not extend far enough into the ultra-violet to permit a complete investigation of the varying appearances of the two ultra-violet enhanced lines to which I have called attention in an earlier part of this communication. Both of them appear in the spectrum of Sirius, however, which is certainly a hot star, though not one of the hottest.

Still the facts are sufficient to show that although H and K appear in some of the stars without any traces of the other calcium lines, there can be no hesitation in accepting them as due to calcium, as in all such stars a very high temperature is independently indicated by the great extension of the continuous spectrum into the ultra-violet and by the iron lines previously discussed.

Magnesium.

Magnesium furnishes us with very definite indications of the four stages of temperature, and the discussion of its representative lines in stellar spectra is therefore very important.

In α Orionis, the *b* group is strongly developed, while 4481 is absent. In the map, the ultra-violet triplet commencing with 3834 is taken as typical of the second stage spectrum, but the photographs of the spectrum of α Orionis which have been obtained do not extend far enough to enable the presence or absence of this triplet to be ascertained. Another line, at 4352·2, which appears at the second stage of temperature is, however, probably present in α Orionis.

The flame line 4571·3 is also present,* and I have also shown the probable presence of the two flutings of magnesium in the spectrum of α Orionis and similar stars.† In α Orionis, then, the most effective absorbing magnesium vapour is at a temperature not greatly different from that of the flame, but the presence of 4352·2 indicates that the temperature must be slightly greater. It is important to remember, however, that on the meteoritic hypothesis, different parts of such a swarm as α Orionis may have widely different temperatures, as some of the collisions may be end-on while others may be mere grazes, so that a mixed spectrum of high and low temperature lines might be expected.

Taking γ Cygni, as before, to be considerably hotter than α Orionis, as indicated not only by the length of continuous spectrum but by the iron and calcium results, there is a considerable change in the lines representative of magnesium. The flutings have quite disappeared and the spark line at 4481 appears as a well-marked line.

* 'Phil. Trans.,' 1893, vol. 184, A, Plate 28.

† 'Roy. Soc. Proc.,' vol. 44, p. 54.

Some of the arc lines, including 4352·2 and 4167·8 remain, so that in the absence of a record of the ultra-violet triplet, it may be concluded that the absorbing magnesium in γ Cygni is probably at a temperature not differing greatly from that of the spark, that is the third stage of temperature. The wave-length of the new spark line about 4395 is not yet known with sufficient accuracy, on account of its great breadth, to justify its use in this inquiry. The actual appearance of the magnesium lines in γ Cygni thus confirms the conclusion with regard to the temperature of this star which has already been derived from the discussion of the lines of calcium and iron.

In the case of α Cygni the most prominent magnesium line is the spark line at 4481. The *b* lines, the ultra-violet triplet commencing with 3838·4, and the line 4352·2 are also present, while 4571·3 and 4167·8 are absent, or very feeble. The great intensity of 4481, which is only a short line in the spark, indicates that the temperature is, in all probability, a little higher than that of the experimental spark, that is, intermediate between the third and fourth stages.

Passing to η Leonis, 4481 is a little less intense than in α Cygni, while the line at 4352 is considerably reduced in intensity as compared with α Cygni. A temperature a little higher than that of α Cygni is therefore indicated.

In Rigel, where the temperature is higher than in η Leonis, 4481 is one of the few strong lines recorded in the spectrum, and it appears without the other Mg lines.

The same is true of ζ Tauri and Bellatrix, except that 4481 is now reduced in intensity. These varying appearances are indicated in Map I.

The study of magnesium thus perfectly accords with what we have learned as to relative stellar temperatures from a discussion of the lines of iron and calcium.

General Results with regard to Calcium and Magnesium.

Map I indicates that in the case of the stars so far discussed, the same order of temperature is arrived at by a consideration of the lines of calcium and magnesium as that deduced in the first instance from the relative lengths of continuous spectrum, and afterwards by an inquiry into the presence of the enhanced iron lines. Four indications of stellar temperatures are therefore now available, namely, the extent of the continuous radiation, the lines of iron, the lines of calcium, and the lines of magnesium.

The enhanced lines of calcium and magnesium, unlike those of iron, do not disappear from the spectrum in the case of the hottest stars yet studied, but they become very feeble, so that an approach to disappearance is indicated.

V. On the Occurrence of the Lines of the Cleveite Gases and of Hydrogen in the Hottest Stars.

Important Position of the Cleveite Gases.

So far I have considered the high temperature lines of certain metallic elements, and their comparison with lines existing in the stars. It is now important to refer to the permanent gases, which we now know to exist, both in stars and nebulæ. I refer to hydrogen and the cleveite gases. These appear in the stars which, on the grounds previously stated, I hold to be of highest temperature, and for this discrimination we can rely better on the cleveite gases than on hydrogen, for the reason that the latter is much more widely distributed *quâ* temperature. There are not many groups of stars which do not indicate the presence of hydrogen, while, on the other hand, the cleveite gases only occur conspicuously in one.

It is impossible to over-estimate the importance of the discovery of terrestrial sources of helium and the gas I have provisionally named gas X* in its bearings upon the spectra of the hotter stars, since it explains many of the strongest lines in such spectra, as I showed in papers communicated to the Royal Society on May 8 and May 9, 1895.†

Professor Vogel arrived, afterwards, at the same conclusion.‡

Thus a considerable number of the stellar lines was removed from the category of "unknown lines."

Varying Intensity of the Cleveite Lines.

If the varying intensities of the helium lines are studied in the case of the stars which have so far been considered, it is found that the lines become stronger as the temperature increases. Thus, from the merest trace in α Cygni the lines of helium are gradually intensified in passing through η Leonis, Rigel, ζ Tauri, and Bellatrix, which, as we have already seen, are in order of increasing temperature. This is illustrated in Map II, which also shows the behaviour of the lines belonging to that constituent of the cleveite gases which I have provisionally called gas X.

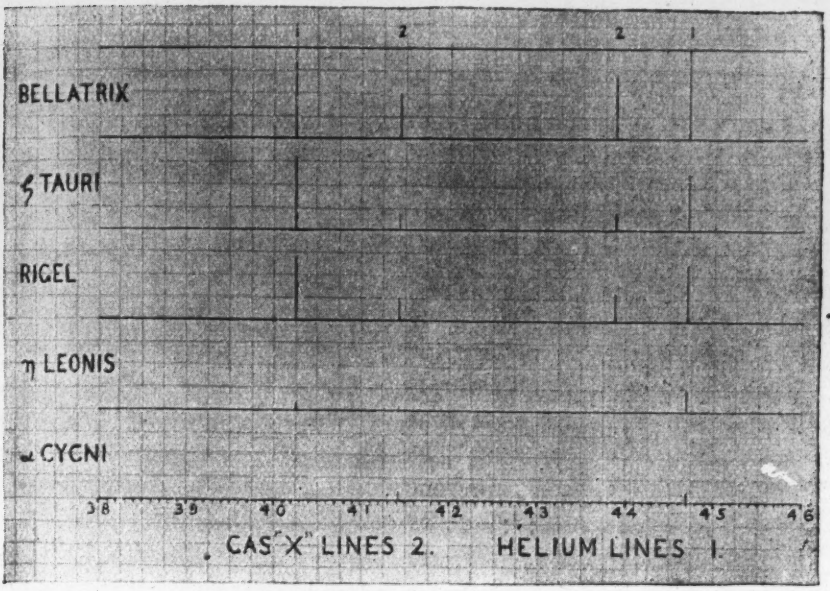
Varying Intensity of the Lines of Hydrogen.

The lines of hydrogen make their appearance at a much lower stage of temperature than those of the cleveite gases, but, like the

* For greater clearness in the sequel, and because the lines of helium are stronger than those of gas X, I shall often refer to helium alone.

† 'Roy. Soc. Proc.,' vol. 58, pp. 114, 117.

‡ 'Berlin Sitzungsberichte,' October 24, 1895.



MAP II.—Behaviour of lines of cleveite gases in stars of increasing temperature.

helium lines, they steadily increase in intensity up to the highest temperature in the case of the stars already discussed. At all stages where the lines of helium and hydrogen appear together, the hydrogen lines are the stronger, so far as our present information goes, but they do not reach their maximum intensity in any of the stars we are now discussing.

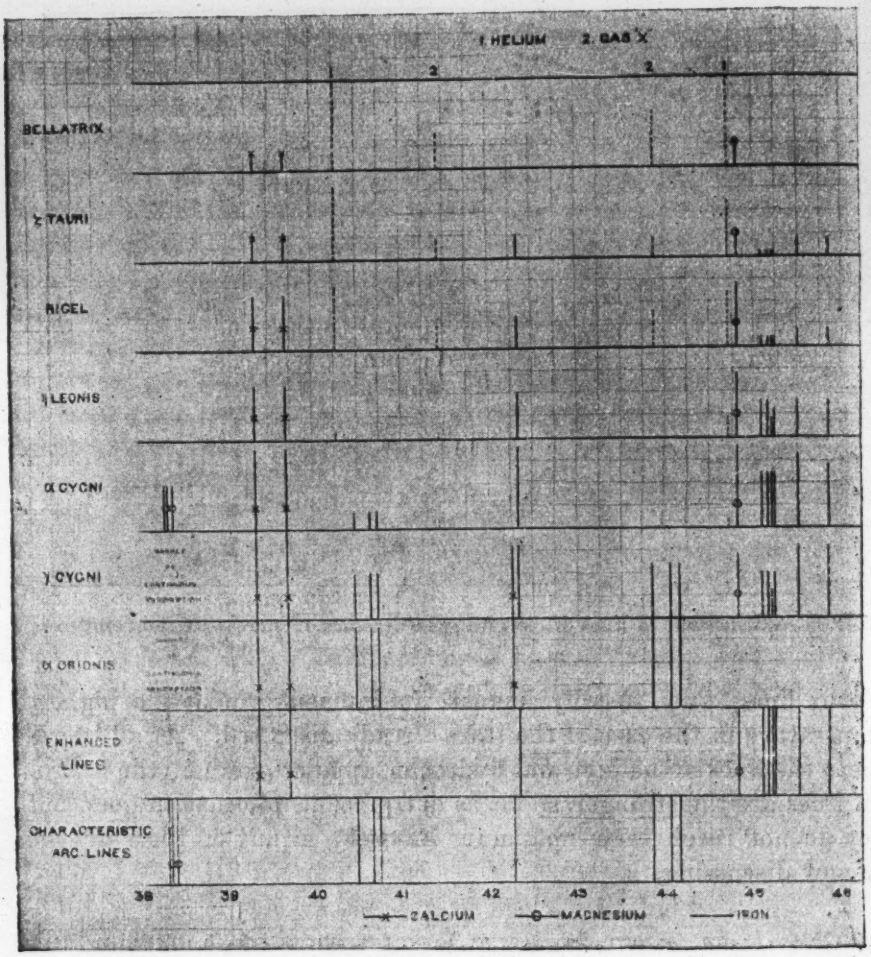
VI. COMPARISON OF THE METALLIC RESULTS WITH THOSE GIVEN BY THE CLEVEITE GASES AND HYDROGEN.

The Cleveite Gases.

Dealing with the stars already considered, a comparison of the metallic and cleveite gas lines indicates that as the former die out the latter are strengthened.

The cleveite gas absorption first makes its appearance, very feebly, in the stars in which the enhanced lines of magnesium and iron are strongest, as in α Cygni. Then, as the temperature increases, as demonstrated by the expansion into the ultra-violet of the continuous radiation, the lines of the cleveite gases become stronger as the metallic lines thin out.

At the highest temperature, taking Bellatrix for the present as a typical case, the principal helium lines are almost as strong as the lines of hydrogen, while the enhanced lines of iron have quite disappeared, and those of magnesium and calcium nearly so. These variations are indicated in Map III.



MAP III.—Comparison of metallic lines with lines of cleveite gases in stars of increasing temperature.

It will be seen from the map that there is a perfect continuity both in the case of the metallic and the cleveite gas lines, and that in those stars where the separate investigations of the metallic and gas lines overlap, the same arrangement of stars results in both cases, *but there is a complete inversion in the behaviour of the gas as compared with the metallic lines.* We seem to be in presence of a chemical change, iron being finally replaced by helium.

Hydrogen.

The dark lines of hydrogen make their appearance in stars of comparatively low temperature, such as α Orionis, in which the second stage metallic lines are very strong. In these stars the hydrogen lines are thinner than some of the metallic ones.

With increased temperature, in the case of the stars so far considered, the lines of hydrogen steadily increase in intensity up to the highest temperature, as typified by Bellatrix. Meanwhile, as we have seen, the second stage metallic lines gradually thin out, while the enhanced lines become stronger up to the temperature of α Cygni, and ultimately all the lines of iron have disappeared, and magnesium and calcium are only represented by traces of their enhanced lines.

When the lines of the cleveite gases first become visible, as in α Cygni, the lines of hydrogen have already become the strongest in the spectrum, but at still higher temperatures, the intensification of the lines of hydrogen is less rapid than that of the lines of the cleveite gases. In none of the stars now considered do the hydrogen lines reach their maximum development, as stated before. This point will be considered later on.

VII. The Temperature Ranges of the Metallic and Cleveite Gas Lines.

Since there are regular sequences in the intensities of both the metallic and cleveite gas lines in passing through stars of gradually increasing temperature, it is important to compare the behaviour of the lines of the different substances between the limits of the temperature range I have chosen for discussion.

The metallic lines characteristic of the fourth stage of temperature behave differently in the case of different metals, although all the comparisons have led to the same general conclusion with regard to the temperature of the absorbing vapours in any particular star.

The Lower Limit in the case of the enhanced Metallic Lines.

In the case of calcium, the enhanced lines, namely H and K, appear faintly at the temperature of the oxy-hydrogen flame (Stage 1), and are strongly developed at the arc temperature (Stage 2). The appearance of H and K as strong lines in α Orionis is therefore to be expected.

In the case of iron, the enhanced lines do not occur at all at the first stage of temperature, and only very feebly at the second. They would therefore not be expected at the low temperature of α Orionis, and, as a matter of fact, unlike H and K they first appear as well developed lines in α Cygni.

Magnesium behaves almost exactly like iron in regard to its enhanced line (4481), since, absent from α Orionis, it appears as a prominent line in α Cygni.

The Upper Limit in the case of the enhanced Metallic Lines.

A somewhat similar difference of behaviour is noted when the phenomena at the higher temperature limit are studied. The enhanced lines of calcium and magnesium do not quite vanish even when the temperature of Bellatrix is reached, but they become very thin. With iron the case is different; the enhanced lines vanish entirely in Bellatrix, and are very much thinner than the enhanced lines of calcium and magnesium in the spectrum of Rigel.

From this it will be seen that H and K appear even at the first stage of temperature, and survive up to the upper limit of Stage 4. The magnesium line, 4481, does not appear until the third stage of temperature is reached, but survives to the upper limit of the fourth stage. In the case of iron, the enhanced lines, like that of magnesium, do not appear until the third stage is reached, but, unlike it, they do not survive the upper limit of the fourth stage.

The Cleveite Gases.

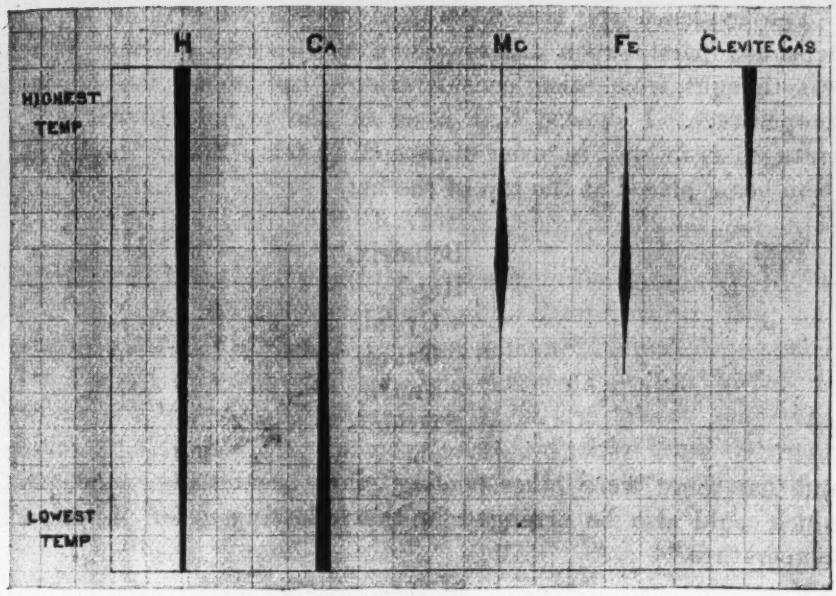
The lines of the cleveite gases do not appear in stellar spectra until the temperature is high enough to exhibit the enhanced lines of magnesium and iron with their greatest intensity. At this stage the gas lines appear only very feebly, but, unlike the metallic lines, they gradually increase in intensity up to the highest temperature, as represented by Bellatrix.

The range of the cleveite gas lines is therefore shorter than that of the enhanced lines of iron and magnesium, and the order of intensity is inverted.

It is important to note, and I shall return to this point, that while the cleveite gases are represented by absorption lines in the spectra of the hotter stars only, hydrogen gives visible indications of its presence in stars at nearly all stages of condensation.

General Conclusions.

These observations indicate individual peculiarities of the spectra of the different substances. Some appear to be longer lived than others, and, further, the important spectral changes in the case of different substances do not occur at the same temperature The differences of behaviour in the case of the enhanced lines of iron, magnesium, calcium, hydrogen, and the cleveite gases are illustrated diagrammatically in Map IV. It will be seen from the map that there are two substances with a long range, calcium and hydrogen, the former being succeeded by the latter as the temperature increases; one of intermediate range, magnesium, which remains visible up to the highest temperature, and two of



Map IV.—Temperature ranges of different substances.

shorter range, iron and the cleveite gases; the enhanced lines of the former being interpolated about half way up the long stages of calcium and hydrogen, and the lines of the cleveite gases near the top.

The map also indicates the relative temperatures at which the changes in the spectra of different substances take place.

With special regard to iron, it may be noted that the fundamental change in its spectrum occurs at about the temperature of α Cygni, while the enhanced lines of calcium are unaffected at that temperature.

VIII. Inversion of the Phenomena on Cooling in the case of the Metallic and Cleveite Lines.

The Temperature Curve.

In my previous investigations, both visual and photographic, of stellar spectra, I was led to conclude, in opposition to Vogel's view, that some stars are becoming hotter, while others are becoming cooler.*

I first suggested a temperature curve in 1887,† on the left side of which are placed stars of increasing temperature, while the cooling stars occupy the right hand side, the highest temperature occupying the highest part of the curve.

* 'Roy. Soc. Proc.,' vol. 43, p. 145; *ibid.*, vol. 45, p. 309; 'Phil. Trans.,' A, vol. 184, p. 709.

† 'Roy. Soc. Proc.,' vol. 43, p. 144.

The evidence for this separation of the stars into two series depended partly upon differences in the spectra of stars which, it was thought, from other considerations, had about the same mean temperature. I showed that some of the principal stars might be arranged as follows, in order of ascending temperature, the hottest of hem being placed at the top of the list,

Bellatrix,
Rigel,
α Cygni,
γ Cygni,
α Tauri,
α Orionis,

and that there were other cooling stars besides our sun, some of which could also be arranged in the following order of descending temperature.*

Sirius,
β Arietis,
Procyon,
Capella,
Arcturus (Sun).

Arcturus is bracketed with the sun, because I have shown that its spectrum is like that of the sun, line for line.†

The later discussion of the Kensington photographs of stellar spectra fully justified the classification of stars suggested by the previous inquiry into the visual spectra, and the general view that there are bodies of increasing as well as bodies of decreasing temperature was also greatly strengthened.

Metallic Results, Descending.

In what has gone before, I have dealt only with stars of increasing temperature, and more particularly with those near the top of the temperature curve.

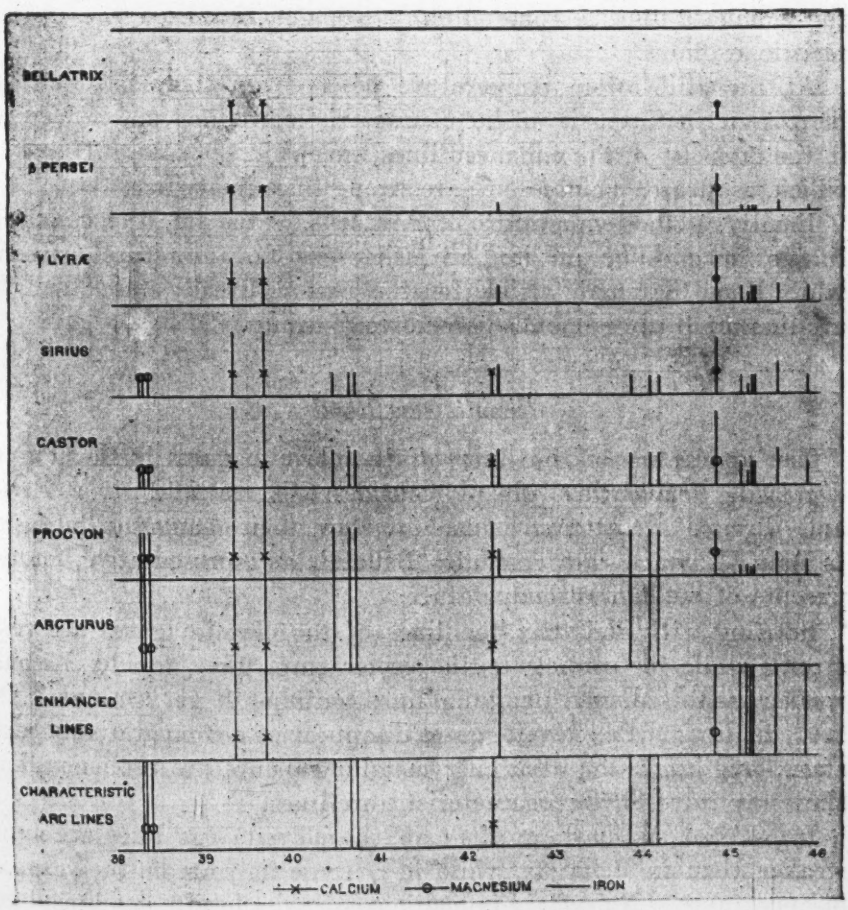
I now pass to the discussion of those stars which the previous investigations indicated to be cooling bodies.

The stars included in the inquiry are as follows, the coolest being placed at the bottom of the list, Bellatrix, for the present being taken as representative of the hottest stars :—

* 'Phil. Trans.,' vol. 184, A, p. 709.
† *Ibid.*, vol. 184, A, p. 699.

Bellatrix,
β Persei,
γ Lyræ,
Sirius,
Castor,
Procyon,
Arcturus.

Map V indicates the variations of the metallic lines in this series of stars, and it will be seen that at the highest temperature magnesium and calcium are represented by their enhanced lines alone; with a fall of temperature the enhanced lines of iron are next added, and, later, the arc lines of all three metals; at the lowest temperature, the enhanced line of magnesium disappears, while those of iron are either absent or very weak; the enhanced lines of calcium, on the other hand, being relatively strong in the arc, remain.



MAP V.—Behaviour of metallic lines in stars of decreasing temperature.

These phenomena are illustrated in Map III by photographs of the spectra of typical stars.

The facts recorded in the photographs of each star are as follows:—

Starting with Bellatrix, magnesium is solely represented by the enhanced line at 4481, which is relatively feeble, calcium by the H and K lines, also feeble, while there are no indications of iron at all.

Passing next to β Persei, 4481 and the H and K lines are intensified, while the enhanced lines of iron are added.

In γ Lyræ, all the enhanced lines of magnesium, calcium, and iron are strengthened.

A further intensification of the enhanced lines occurs on passing to Sirius, but here the characteristic arc lines of all three substances are added, indicating a temperature intermediate between that of the experimental spark and that at which the enhanced lines appear alone.

With a further slight fall of temperature, represented by Castor, the enhanced lines become slightly stronger, as do also the characteristic arc lines.

At the still lower temperature of Procyon there is a marked increase in the intensity of the characteristic arc lines and a decrease in the intensity of the enhanced lines, except in the case of H and K, which, as already pointed out, are strong lines in the arc.

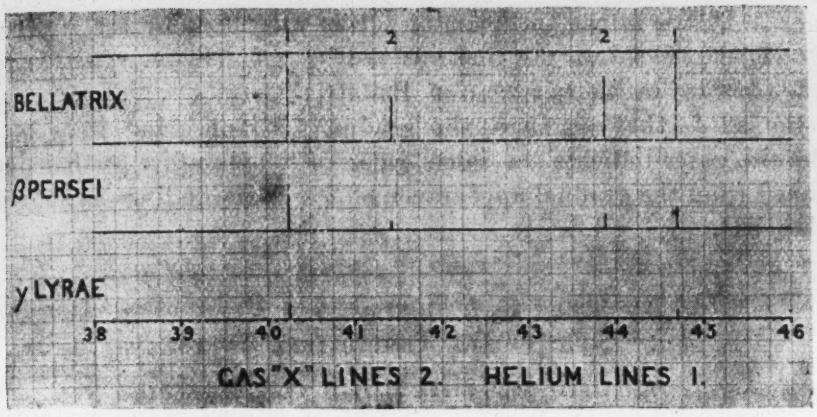
Finally, at the temperature of Arcturus, or the sun, the enhanced lines of magnesium and iron are either very faint or entirely absent, while H and K remain visible for the reason already stated, and the arc lines of all three metals become very strong.

Cleveite Gas Results.

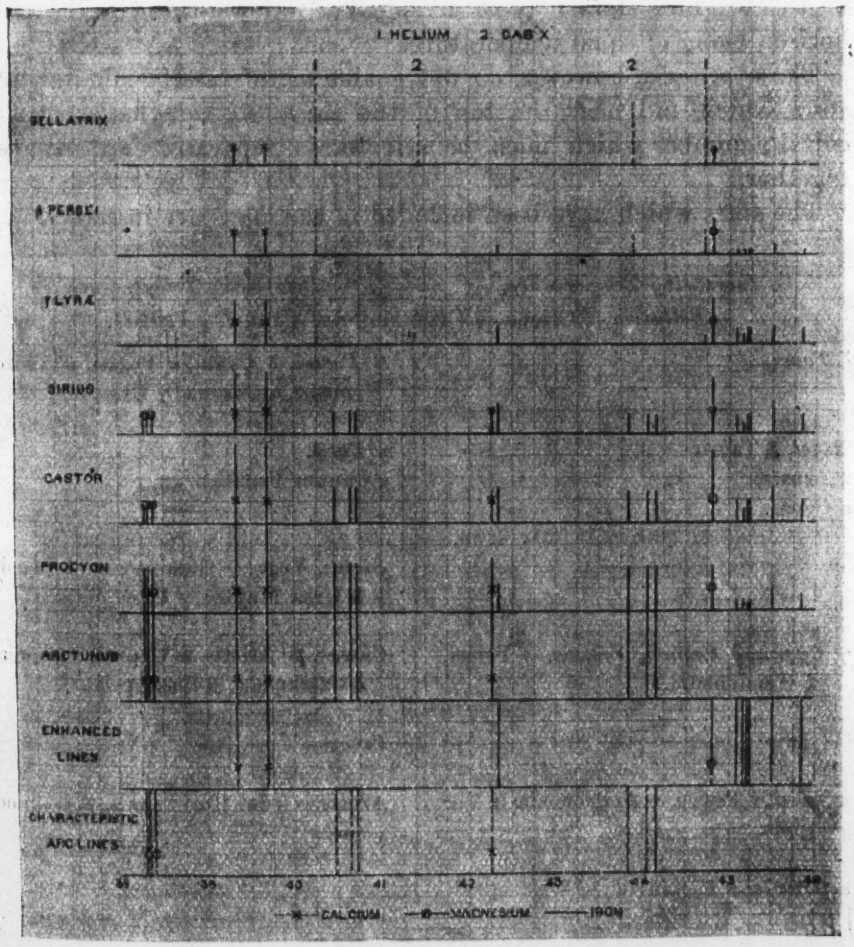
The appearance of the lines of the cleveite gases in the stars of decreasing temperature already considered is restricted to β Persei and γ Lyræ. An attempt is made to show their changes of intensity in Map VI, which also includes Bellatrix as representative, for the present, of the highest temperature.

Starting with Bellatrix, the lines of the cleveite gases are very strong, while, on passing to the cooler stars, they rapidly become weaker, as the enhanced metallic lines continue to get stronger. In fact, the lines of the cleveite gases disappear at a comparatively early stage, long before the absorbing metallic vapours are cool enough to show any traces of the characteristic arc lines.

In β Persei, as shown on the map, the cleveite gas lines are much weaker than in Bellatrix, while in γ Lyræ they are on the verge of disappearance. No certain indications of the cleveite gas lines have been found in Sirius, Castor, Procyon, or Arcturus. A special search



MAP VI.—Behaviour of lines of cleveite gases in stars of decreasing temperature.



MAP VII.—Comparison of metallic lines with lines of cleveite gases in stars of decreasing temperature.

for the D_3 line in the spectrum of Sirius was made by Mr. Fowler in December, 1893, but the line was not seen in this star, although it was recorded in the spectrum of Bellatrix.*

So far as they go, then, the gradually disappearing lines of the cleveite gases indicate the same order of temperature as that determined from the gradual appearance of the metallic lines as shown in Map VII.

IX. On the Absorbing Conditions on the Upward and Downward Sides of the Temperature Curve.

Temperature Order of some of the Hottest Stars on the two Sides of the Temperature Curve.

Utilising the new criteria which have now become available, some of the brighter stars may be arranged as follows, those on the same horizon being of equal temperature.

These criteria, however, do not enable us to classify minutely the stars which fall near the top of the curve, so, for the present, a certain number which must be afterwards separated, are grouped together.

The stars which have been included in the maps are in italics.

Increasing Temperature.	*Decreasing Temperature.*
(*Bellatrix*, ζ Orionis, η Ursæ Majoris, λ Tauri, γ Pegasi).	
ζ *Tauri.*	β *Persei*, δ Cygni, α Pegasi, α Andromedæ, α Coronæ, γ Ursæ Majoris.
Rigel, β Tauri, η *Leonis*, α *Cygni.*	γ *Lyræ.* α Canum Venaticorum.
	Sirius, Vega, γ Geminorum, δ Leonis, β Ursæ Majoris, ε Ursæ Majoris.
γ *Cygni*, δ Cephei, Polaris, α Persei, ζ Geminorum.	*Castor*, β Arietis, α Cephei, α Aquilæ, δ Cassiopeiæ, β Cassiopeiæ.
	Procyon.
α Tauri, ε Pegasi, γ Andromedæ, ε Virginis.	*Arcturus* (Sun), α Arietis, β Geminorum.
α *Orionis*, β Pegasi.	

* 'Phil. Trans.,' vol. 186, A, p. 85.

The Absorptions not Identical.

Early in my researches I pointed out* that the absorption phenomena in stellar spectra need not be identical at the same mean temperature on the ascending and descending sides of the curve, since, on the meteoritic hypothesis, there must be a tremendous difference in the physical conditions.

In a condensing swarm, the centre of which is undergoing meteoritic bombardment from all sides, there cannot be the equivalent of the solar chromosphere; the whole mass is made up of heterogeneous vapour at different temperatures, and moving with different velocities in different regions.

In a condensed swarm, of which we can take the sun as a type, all action produced from without has practically ceased; we get relatively a quiet atmosphere and an orderly assortment of the vapours from top to bottom. But still, on the view that the differences in the spectra of the heavenly bodies chiefly represent differences in degree of condensation and temperature, there can be, *au fond*, no great chemical difference between bodies of increasing and bodies of decreasing temperature. Hence, it is exceedingly probable that at equal mean temperatures on opposite sides of the temperature curve, this chemical similarity of the absorbing vapours will result in many points of resemblance in the spectra.

The identical behaviour of the enhanced metallic and cleveite lines on both sides of the curve shows us that we have here a previously undreamt-of test to apply to this question, and it is important to make sure of this ground before I proceed to apply it to others opened up and rendered clearer by the new investigations.

The New Criteria.

With the photographs at present available, the test can be applied at three stages of temperature, the stars of equal mean temperature being determined by the relative intensities of the enhanced and arc metallic lines. When several stars of the same mean temperature are thus brought together, it soon becomes evident that they are divisible into two groups, which differ considerably in other respects. To take one instance, the average temperature of the absorbing iron vapour is about the same in ϵ Pegasi as in Arcturus, since the spectroscopic difference between these stars, so far as the line spectra are concerned, is very slight. But the continuous absorption in the violet is much greater in ϵ Pegasi than in Arcturus, while the metallic lines are also somewhat broader. The difference between the stars, therefore, does not appear to be due alone to a difference of temperature.

* 'Roy. Soc. Proc.,' vol. 44, p. 26.

Applying this method of separation, some of the typical stars given on the maps exhibit the following characteristics, those of equal temperature, as determined by the metallic lines, being placed on the same horizon.

Ascending Arm.

Rigel:—
1. Long continuous spectrum.
2. Hydrogen lines moderately thick.
3. Metallic lines of moderate intensity and thickness.
   Cleveite gas lines of moderate intensity.

γ Cygni:—
1. Considerable continuous absorption in ultra-violet.
2. Hydrogen lines relatively thin.
3. Metallic lines of moderate intensity.

ε Pegasi:—
1. Strong continuous absorption in violet.
2. Metallic lines thick.

Descending Arm.

γ Lyræ:—
1. Continuous spectrum probably a little longer than in Rigel.
2. Hydrogen lines very thick.
3. Metallic lines weak and thin.
4. Cleveite gas lines very weak.

Castor:—
1. Very little continuous absorption in ultra-violet.
2. Hydrogen lines relatively very thick.
3. Metallic lines relatively feeble.

Arcturus:—
1. Little continuous absorption in violet.
2. Metallic lines of moderate thickness.

General Conclusion as to Spectroscopic Differences on the Two Sides of the Curves.

The facts thus brought together indicate that at each of three very distinct stages of temperature there are two groups of stars showing spectroscopic differences. Generalising from these, it may be stated that stars at about the same temperature, as judged by the iron lines on the ascending side of the curve, differ from those on the descending side.

(1) In the greater continuous absorption in the violet or ultra-violet, especially at the lower stages of temperature.

(2) In the relative thinness of the hydrogen lines at the higher stages of temperature.

(3) In the greater intensity and thickness of the metallic lines, whether of low or high temperature.

(4) In the relatively greater thickness of the lines of the cleveite gases at those stages of temperature in which they appear.

The probable Physical Causes of the Differences.

I submit that the differences found are precisely those we should expect on the meteoritic hypothesis. I deal with the differences indicated *seriatim*.

1. *The Inequality of the Continuous Absorption.*—I have already pointed out that in the case of a *swarm* there must always be cooler vapours mixed with the hotter ones in the most valid absorbing regions. It is these cooler vapours which produce the absorption in the violet and ultra-violet.

In a *condensed mass of vapour* they can only exist at the limit of the atmosphere where their absorption is reduced in consequence of the low pressure.

2. *The smaller thickness of the Hydrogen Lines.*—The difference in thickness of the hydrogen lines is also sufficiently accounted for by the difference of absorbing conditions. In the stars of increasing temperature, consisting of uncondensed meteoritic swarms, the interspaces will be largely occupied by hydrogen at a high temperature, and the radiation of this gas will tend to mask the absorption produced by that in the immediate neighbourhood of the incandescent meteorites which merely graze. In the condensed stars with photospheres, any masking effect of this kind must be very much less pronounced, so that the hydrogen lines will be broader than in the spectra of uncondensed swarms. I have shown in a previous part of this paper that the hydrogen lines are more constant, in regard to their intensities, than the lines of other substances in the stars of increasing temperature, and this greater life of the hydrogen molecule seems to explain the fact that the hydrogen lines are specially picked out after passing to the downward side of the temperature curve when a state of quiescence is reached.

It should be remarked that the great distention of the hydrogen lines in the hotter stars which have begun to cool does not necessarily indicate a great thickness of absorbing hydrogen, since, in the case of the sun, the very broad H and K lines are produced by an absorbing region of small thickness in comparison with the sun's diameter.

3. *The greater thickness of the Metallic Lines.*—In the case of a swarm, the thickness of the effective absorbing gases and vapours in the line of sight will be very much greater than the effective thickness in the case of an atmosphere surrounding a photosphere, even if the masses and average temperatures be the same in both cases. The light proceeding from the central parts of the swarm must pass through the whole depth of vapours filling the interspaces, and on this account the absorption lines would be more intense in the case of a swarm. The greater intensities, and to some extent the breadth of the metallic lines, are thus explained, for since the metallic vapours

will not fill the interspaces to the same degree that the hydrogen does, there will be no masking of the dark lines by radiation.

4. *The Widening and Thinning of certain Lines.*—In consequence of the great difference of velocity and direction of the meteorites entering a swarm, the spectrum lines involved will in general be broader, and therefore dimmer, so far as this cause is concerned, than in stars where bombardment has ceased. Such a broadening was specially noticeable in the spectrum of Nova Aurigæ, as I pointed out in a paper communicated to the Royal Society on February 11, 1892.*

The conditions, however, may vary considerably in different cases, according to the character of the parent nebula. In the case of a swarm which originally had a spiral structure, the chief movement will be very similar to one of rotation; if the axis of rotation be directed towards the earth, such a movement will produce little or no effect on the widths of the lines, but if the axis be not so placed, different amounts of broadening would be produced, according to the inclination and radial velocity. In these cases, lines which would be sharp at the edges when there was no movement, would remain sharp at the edges when broadened and dimmed.

When the original nebula was of less regular form, the influx of meteorites towards the centre will take place in a greater variety of directions, so that the broadening effects will be less regular.

Actions of this kind, in addition to those already referred to in 3, are probably to some extent responsible for the generally greater breadth of the metallic lines in bodies of increasing temperature as compared with those in cooling stars of the same mean temperature.

ζ Tauri is an interesting case in point. While the lines of hydrogen are quite sharp and not very broad, those of the cleveite gases are greatly distended and relatively dim. In this case, therefore, it would appear that the cleveite gases are more involved than hydrogen in the highest temperature collisions. In β Lyræ, also, the bright lines of the cleveite gases are more intense than those of hydrogen, and here we have another indication that these gases are among those chiefly involved in the spectral phenomena at the highest temperature; further, there is direct evidence that there are at least two bodies in the system of β Lyræ, and the variability is probably due to collisions between the outlying meteorites.

It may, on the other hand, be that the lines in the spectra of some cooling stars may be broadened as an effect of rotation, as suggested by Professor Pickering in the case of α Aquilæ. My own photographs show that the spectrum of this star is almost identical with that of β Arietis, except that all the lines are broadened. In this and similar spectra, such as α Ophiuchi and α Cephei, the broadening is accompanied by a reduction in intensity.

* 'Roy. Soc. Proc.,' vol. 50, p. 434.

5. *The greater thickness of the Lines of the Cleveite Gases.*—The action which produces the lines of the cleveite gases, whatever it be, only commences shortly before the highest temperature is reached, and the importance of helium in the spectrum grows very quickly. When the action has ceased, the helium lines rapidly lose their importance, whilst the absorbing hydrogen continues for some time to become more effective.

The complete discussion of these differences cannot be undertaken until the criteria for stars at the apex of the temperature curve have been further investigated.

X. Are we in Presence of the Highest Stellar Temperatures?

I have shown in § IV that our highest terrestrial temperatures only carry us up to a temperature approximately represented by γ Cygni.

We have no right to assume that the small number of stars as yet studied put us in presence of the highest stellar temperatures.

In the present communication I have of set purpose left on one side for future treatment those stars which apparently are at the very apex of the temperature curve, for the reason that in these stars we are involved in unknown lines. These require a special study; other stellar photographs have to be examined, a work which will require some time, and new photographs have to be taken.

In all probability, among the stars already known there are others besides Bellatrix which are hotter than ζ Tauri and Rigel, as determined by the continuous spectrum and by the metallic lines in the way already explained; although the known gas lines are of nearly similar intensity, these stars show distinct differences among themselves, and other criteria for their arrangement in order of temperature must, therefore, be looked for.

At this stage the only remaining known metallic lines are probably magnesium 4481, and the K line of calcium; these, however, are so feeble that their variations are too much dependent on the quality of the photographs to be trustworthy criteria, and the same remark applies also to the measurement of the extension of the spectrum into the ultra-violet. There are, however, several lines, as yet of unknown origin, which are strong in some of these stars and weaker in others, and the appearances of these afford useful criteria for classifying these stars. But, before we attempt to use these lines to see whether any higher temperature than that of Bellatrix is indicated, it is important to consider whether we are justified in regarding these unknown lines as gaseous instead of metallic. This question is now being studied, and until it is settled it is not wise to attempt to discuss the upper temperature limits of those substances, such as hydrogen and the cleveite gases, the lines of which increase

in importance with the increase of stellar temperature in the stars I have so far discussed.

For instance, if such hotter stars were found we might expect that either hydrogen or the cleveite gas lines would thin out and then disappear first, as the iron lines thin out in Rigel and disappear in Bellatrix, and that they would be replaced by other lines.

As a matter of fact, the hottest stars under discussion do contain other lines besides those of hydrogen and the cleveite gases.

XI. Bearing of the New Results on the Dissociation Hypothesis.

§ 1. *Historical Statement.*

It was in 1873 that I first called the attention of the Society to the very remarkable facts which had been even then got together regarding the possible action of heat in the sun and stars. Referring more especially to the classification of stars by Rutherfurd and Secchi, I wrote as follows:—*

"I have asked myself whether all the above facts cannot be grouped together in a working hypothesis which assumes that in the reversing layers of the sun and stars various degrees of 'celestial dissociation' are at work, which dissociation prevents the coming together of the atoms which, at the temperature of the earth and at all artificial temperatures yet attained here, compose the metals, the metalloids, and compounds."

Subsequently in a private letter to M. Dumas, who took the keenest interest in my solar work, I wrote, "Il semble que plus une étoile est chaude plus son spectre est simple."

I also pointed out the close relation of hydrogen to calcium, magnesium, and other metals (it was on this ground that I had named the substance which gave D^3, which always varied with hydrogen, helium), and the absence of all other terrestrial gases from the solar spectrum. An interesting discussion at the Paris Academy of Sciences was thus concluded by M. Dumas:—

"En résumé, quand je soutenais devant l'Académie que les éléments de Lavoisier devaient être considérés, ainsi qu'il avait établi lui-même, non comme les éléments *absolus* de l'univers, mais comme les éléments *relatifs* de l'expérience humaine; quand je professais, il y a longtemps, que *l'hydrogène* était plus près des métaux que de toute autre classe de corps; j'émettais des opinions que les découvertes actuelles viennent confirmer et que je n'ai point à modifier aujourd'hui."†

* 'Phil. Trans.,' vol. 164, Part 2, p. 491 (reproduced in 'Chemistry of the Sun,' p. 201).

† 'The Chemistry of the Sun,' p. 205.

At that time (1873) no fluted metallic spectra, with the exception of hydrogen, had been recognised, and I stated that my work had not revealed any.* From that time to the present the area of investigation has been extended by leaps and bounds: hundreds of thousands of facts have been accumulated. I propose, therefore, to inquire in this concluding part of my communication whether my early attempt to group the facts available nearly a quarter of a century ago still holds good.

Before, however, I refer to the results of the new work contained in this memoir, having special reference to the stars, it will be necessary to give a very hasty sketch of some of the main conclusions derived from solar research, the more especially since the stellar work was taken up with a view of testing the views arrived at from a special study of the sun.

When I began spectroscopic work in 1865, the general idea was that the spectrum of a chemical substance was one and indivisible—that it could not be fundamentally changed by temperature.

In a paper communicated to the Royal Society† in 1874, I alluded to the evidence showing that this opinion was erroneous, and that metals, including hydrogen and, as recently had been shown,‡ potassium and sodium possessed fluted spectra, the line, fluting, and continuous spectra indicating different molecular complexities which could be dissociated by heat and electricity.

In 1878 I went further,§ and showed that the solar phenomena could only be explained by assuming that the changes in the various intensities of lines in the line spectrum itself indicated successive dissociations. I pictured the effect of furnaces of different temperatures, and I wrote as follows:—

"It is abundantly clear that if the so-called elements, or, more properly speaking, their finest atoms—those that give us line spectra—are really compounds, the compounds must have been formed at a very high temperature. It is easy to imagine that there may be no superior limit to temperature, and therefore no superior limit beyond which such combinations are possible, because the atoms which have the power of combining together at these transcendental stages of heat do not exist as such, or rather they exist combined with other atoms, like or unlike, at all lower temperatures. Hence association will be a combination of more complex molecules as temperature is reduced, and of dissociation, therefore, with increased temperature, there may be no end."‖

* 'Phil. Trans.,' 1873, p. 652, vol. 163, Part 2.
† 'Roy. Soc. Proc.,' vol. 22, p. 374.
‡ *Ibid.*, vol. 22, pp. 362 and 376.
§ *Ibid.*, vol. 28, p. 157.
‖ *Ibid.*, vol. 28, p. 169.

In the same year I also studied the changes in the line spectra in relation to the changes observed when known compounds were dissociated, and after discussing certain objections I submitted the conclusion that the known facts with regard to the changes in line spectra "are easily grouped together, and a perfect continuity of phenomena established on the hypothesis of successive dissociations analogous to those observed in the cases of undoubted compounds."*

Special Reference to Solar Work.

Neglecting the flutings in the extreme red, which are due to our own atmosphere, the only fluting known to exist in the solar spectrum is one which I ascribed to carbon in 1878.†

Hence the solar work has had mainly to do with line spectra. With regard to these, the following anomalies on the ordinary view are given as specimens of the many which have been recorded.

1. Judging by the lines seen in their spectra, the order in which the elements thin out as the cooler parts of the sun's atmosphere are reached has no relation to their atomic weights, either old or new. This order is

Ca
H
He
Sr
Mg
Fe
Ti
Mn

2. Inversion of intensity of lines seen under different circumstances.‡

I showed in 1879 that there was no connexion whatever between the spectra of calcium, barium, iron, and manganese and the chromosphere spectrum beyond certain coincidences of wave-length. The long lines seen in laboratory experiments are suppressed, and the feeble lines exalted in the spectrum of the chromosphere. In the Fraunhofer spectrum, the relative intensities of the lines are quite different from those of coincident lines in the chromosphere.

3. The simplification of the spectrum of a substance at the temperature of the chromosphere. To take an example, in the visible region of the spectrum, iron is represented by nearly a thousand Fraunhofer lines; in the chromosphere it has only two representatives.

* 'Roy. Soc. Proc.,' vol. 28, p. 179.
† *Ibid.*, vol. 27, p. 308.
‡ *Ibid.*, vol. 28, p. 428.

4. In sun spots we deal with one set of iron lines, in the chromosphere with another.

5. At the maximum sun spot period the lines widened in spot spectra are nearly all unknown; at the minimum they are chiefly due to iron and other familiar substances.

6. The up-rush or down-rush of the so-called iron vapour in the sun is not registered equally by all the iron lines, as it should be on the non-dissociation hypothesis. Thus, as I first observed in 1880,* while motion is sometimes shown by the change of refrangibility of some lines attributed to iron, other adjacent iron lines indicate a state of absolute rest.

Laboratory work without stint has been brought to bear, with a view of attempting to explain the anomalies to which attention has been directed.

It is desirable to refer briefly to some of the work undertaken in relation to the anomalies in question.

1. Experiments on calcium to determine cause of inversion of the intensities of H and K.

2. Experiments on magnesium to explain the inverted intensities of the lines in the Fraunhofer spectrum.

3. Experiments on iron to explain the Italian observations of the chromosphere which proved the presence of only two lines of iron in the part of the spectrum ordinarily observed.

Some Special Experiments.

Calcium.—In 1876 I produced evidence that the working hypothesis that the molecular grouping of calcium which gives a spectrum having its principal line at 4226·9 is nearly broken up in the sun, and quite broken up in the spark, explained the facts, which are that the low temperature line loses its importance and practically disappears from the spectrum of the sun, in which H and K are by far the strongest lines.†

I summed up the facts regarding calcium as follows:‡ "We have the blue line differentiated from H and K by its thinness in the solar spectrum while they are thick, and by its thickness in the arc while they are thin. We have it again differentiated from them by its absence in solar storms in which they are almost universally seen, and, finally, by its absence during eclipses, while the H and K lines have been the brightest seen or photographed."

I afterwards attempted to carry the matter further by photographing the spectra of sun spots. In all cases H and K lines were seen

* 'Roy. Soc. Proc.,' No., 207, 1880.

† *Ibid.*, vol. 24, p. 352.

‡ *Ibid.*, vol. 28, p. 171.

reversed over the spots, just as Young saw them at Sherman, while the blue calcium line was not reversed.* The oldest of these photographs which has been preserved bears the date April 1, 1881.

The experimental results in the case of calcium, therefore indicated that the cause of the inversion of intensities in the lines of a substance under different circumstances is due to the varying degrees of dissociation brought about by different temperatures.

Magnesium.—I have already referred to the results which I obtained in 1879,† by passing a spark through a flame charged with different substances. In the case of magnesium the effect of the higher temperature of the spark was very marked, some of the flame lines being abolished, while two new ones made their appearance, one of them at 4481. The important fact was that the lines special to the flame did not appear among the Fraunhofer lines, while those of the spark did appear.

Here again the experiments pointed to varying degrees of dissociation at different temperatures as the cause of the non-appearance of some of the magnesium lines in the Fraunhofer spectrum.

From these experiments the results of which were subsequently mapped in relation to the various heat-levels indicated by solar phenomena, I drew the following conclusions in 1879:—

"I think it is not too much to hope that a careful study of such maps, showing the results already obtained, or to be obtained, at varying temperatures, controlled by observations of the conditions under which changes are brought about, will, if we accept the idea that various *dissociations* of the molecules present in the solid are brought about by different stages of heat, and then reverse the process, enable us to determine the mode of evolution by which the molecules vibrating in the atmospheres of the hottest stars *associate* into those of which the solid metal is composed. I put this suggestion forward with the greater confidence, because I see that help can be got from various converging lines of work."‡

Iron.—The anomalies which have been noted in the case of iron led me to undertake some experimental work with the sparks produced by quantity and intensity coils with and without jars in the circuit.§ The outcome of these experiments was to show that the chromospheric representatives of iron were precisely the lines which were brightened on passing from the arc to the spark, while the lines widened in spots corresponded to a lower temperature.

Attention was at the same time drawn to the gradual simplification of the iron spectrum by increased temperature as we pass from the arc through the spots to the prominences.

* 'Roy. Soc. Proc.,' vol. 36, p. 444.

† *Ibid.*, vol. 30, p. 26.

‡ *Ibid.*, vol. 30, p. 30

It seemed perfectly clear then that in the sun "we were not dealing with iron itself, but with primitive forms of matter contained in iron, which are capable of withstanding the high temperature of the sun, after the iron observed as such, has been broken up, as suggested by Brodie."*

On this view, the high temperature iron lines of the chromosphere represent the vibrations of one set of molecules; while the lines which are widened in spots correspond to other molecular vibrations. Similarly, the idea of different molecular groupings provides a satisfactory explanation of the varying rates of movement of iron vapour indicated by adjacent lines, the lines being produced by absorption at different levels and at different temperatures.

§ 2. *How the Hypothesis has fared.*

The Main Points.

Only some of the views have received general acceptance, those referring to the breaking up of the solid metal giving a continuous spectrum into smaller molecular groupings giving fluted and line spectra.

My view as to the subsequent dissociation of molecules, when once the line spectrum stage has been reached, is still rejected by many. For myself, I am not surprised at this. In a question of such transcendent importance, caution must be redoubled; an absence of work and expression of opinion in such a line of inquiry with questions of pure science only involved, is almost inherent to the nature of the investigations.

The chemist has little interest in an appeal to celestial phenomena, and astronomers do not generally concern themselves with chemistry, The region investigated by the chemist is a low temperature region dominated by monatomic and polyatomic molecules. The region I have chiefly investigated is a high temperature region, in which mercury gives us the same phenomena as manganese. In cases where the two regions overlap, vapour density determinations and other work have been in harmony with the spectroscopic results, *e.g.*, the changed density of iodine at changed temperatures.

Another, but less direct, argument in favour of dissociation, independently of the changes in the intensities of the lines, was based upon some observations I had made in an attempt to work out a spectroscopic method for the detection of impurities. I noted the presence of what I termed "basic lines," that is, short lines which remained common to two or more spectra, after "long lines" had been eliminated as being due to impurities.

I now refer to these different points *seriatim*.

* 'Roy. Soc. Proc., vol. 32, p. 204.

Flutings represent Vibrations of Complex Molecules.

I take the change of the continuous spectrum successively into flutings and lines first, and in justification of the statement that in this matter my view is now generally accepted, I give the following quotations from Schuster and Eder and Valenta:—

"That the discontinuous spectra of different orders (line and band spectra) are due to different molecular combination I consider to be pretty well established, and analogy has led me (and Mr. Lockyer before me) to explain the continuous spectra by the same cause; for the change of the continuous spectrum to the line or band spectrum takes place in exactly the same way as the change of spectra of different orders into each other.*

"Später führte Lockyer weiter aus, dass die Gase, solange ihre Moleküle aus mehreren Atomen zerfallen, Linienspectren geben müssen. Diese Anschauung wurde seither ziemlich allgemein acceptirt."†

The Complexity of the Line Spectrum.

With regard to the view that the line spectrum integrates for us the vibrations of several sets of molecules, as I have already stated this is not accepted. The number of objections is legion, and it is impossible to refer to all of them here. But, at the same time, the opinion of some of those workers who have approached the subject from both points of view is, I think, coming round to my side, and I shall briefly refer to one or two instances, the first bringing confirmation from a new line of inquiry.

The effect of Pressure on Wave-length.

Messrs. Humphreys and Mohler have recently made, at the Johns Hopkins University, a series of experiments upon the effects of pressure on the wave-lengths of metallic lines.‡ Using an electric arc inclosed in a cast-iron cylinder, they have observed the spectra at pressures varying up to fourteen atmospheres.

The general effect of pressure was to broaden the lines and to bring out the reversals, but cadmium was found to be an exception. With one exception, all the measurements actually made of the spectra of twenty-three elements showed that "the shifts were invariably towards the less refrangible, *i.e.*, the red end of the spectrum, and that they were directly proportional, not only to the

* Schuster, 'Phil. Trans.,' 1879, Part 1, vol. 169, p. 39.

† Eder and Valenta, 'Denkschriften der kaiserlichen Akademie der Wissenschaften,' Wien, Bd. 61, p. 426, 1894.

‡ 'Astrophysical Journal,' vol. 3, p. 114.

wave-length, but also to the excess of pressure above one atmosphere." The exception referred to is that of calcium, in which case the H and K lines, among others, were shifted only about half as much as the blue line at 4226·9.

Referring to the exceptional behaviour of calcium, Professor Hale points out that the result "seems to support Lockyer's views as to the dissociation of calcium in the arc and sun,"* and further states: "It would thus appear that the temperature of the dissociation of calcium is between that of the Bunsen burner and that of the oxy-coal gas flame. The high molecular weight of calcium has hitherto conflicted with our belief in the presence of this metal in prominences. If, however, it be granted that dissociation can be brought about by temperatures even lower than that of the arc, the difficulty is very greatly lessened."

In an article which I wrote in 'Nature' on this work,† I pointed out that "it would be very interesting to see if the strontium line at 4607 behaves like the calcium *g* in relation to the lines at 4077 and 4215, representing H and K."

This prediction has been confirmed in a paper by W. J. Humphreys,‡ on "The effect of Pressure on the Wave-lengths of lines in the Spectra of certain Elements," in which the author gives a table of the shifts measured on the strontium lines mentioned above. When working with pressures varying from 6 to 12 atmos., the shift of the line at 4077 was always approximately half that at 4607.

There can be little doubt that other enhanced lines will follow suit.

The Question of Series.

The other branch of new work to which I refer is the investigation of "series" of lines in spectra, which was practically started by Balmer in 1885, and in which, since 1888, we owe so much to the labours of Messrs. Kayser and Runge and Rydberg.

I stated in 1878 that the spectrum of a substance was the integration of the spectra of various molecular groupings.

Messrs. Runge and Paschen first showed in 1890§ that the spectra of lithium, sodium, and potassium were the integration of the spectra of various "series." Later they have shown that lead and other metals follow suit.

Before Balmer's formula was employed, rhythmic structure in spectra had often been noticed. Thus, in 1869, Mascart wrote as follows:—

* 'Astrophysical Journal,' vol. 3, p. 160.

† 'Nature,' vol. 53, p. 416, March, 1896.

‡ 'Astrophysical Journal,' vol. 4, p. 249, November, 1896.

§ 'Abh. d. k. Akad. d. Wiss.,' Berlin, 1890.

"Il semble difficile que la reproduction d'un pareil phénomène soit un effet du hasard : n'est-il pas plus naturel d'admettre que ces groupes de raies semblables sont des harmoniques qui tiennent à la constitution moléculaire du gaz lumineux ? Il faudra sans doute un grand nombre d'observations analogues pour découvrir la loi qui régit ces harmoniques."

I wrote thus on this subject in 1879 :—

"I am at present engaged in investigating this question of rhythm, and I have already found that many of the first order lines of iron may probably arise from the superposition or integration of a number of rhythmical triplets. All this goes to show how long the series of simplifications is that we bring about in the case of the so-called elementary bodies by the application of a temperature that we cannot as yet define.

"Indeed, the more one studies spectra in detail, and especially under varying conditions of temperature which enable us to observe the reversal now of this set of lines, now of that, the more complex becomes the possible origin. Some spectra are full of doublets; others, again, are full of triplets, the wider member being sometimes on the more, sometimes on the less, refrangible side. Doublets and triplets, as a rule, reverse themselves more freely than the irregular lines in the same spectrum—which particular doublet or triplet will reverse depending upon the temperature."*

A series of spectral lines may be defined as a sequence of lines, the intensity of which decreases with the wave-length, and the number of vibrations of which may be determined by the formula—

$$A+B/n^2+C/n^4,$$

where n is given the integers from three upwards, and the constants A, B, and C are determined for each element separately.

The fact that lines must close up to one another, as the violet end of the spectrum is reached, indicates that the character of a "series" is best brought under notice in the ultra-violet end of the spectrum. In the visible part of the spectrum the lines forming "series" are too far apart to be recognised as belonging to series.

As soon as it becomes apparent that a set of lines in the violet seems to form a series, computation will at once give the lines that belong to it in the visible part of the spectrum.

So far there has been no definite pronouncement touching the question of temperature or the possibility that each series may represent vibrations of similar molecules, but this question has recently been advanced in an instance which to many will carry conviction with it.

* "On Young's List of Chromospheric Lines," 'Roy. Soc. Proc.,' vol. 28, March, 1879.

Professor Pickering announced last month that the star ζ Puppis contained, in addition to the usual series of hydrogen lines, a second series of rhythmical lines, which he supposed to be due to a new element. On further inquiry, it was found that the lines were closely allied to the hydrogen series, and that it was probably due to that substance under conditions of temperature or pressure as yet unknown.

His basis for this statement was founded on the fact that the wave-lengths of the new series could be directly computed by substituting for the value of n in the formula which gave us the known hydrogen series even numbers instead of odd ones.*

Now, this new series of hydrogen has, so far as we know, only been seen in one other star, namely, 29 Canis Majoris.

Professors Pickering and Kayser both concede that this new form of hydrogen is due most probably to a high temperature, so that we should expect to find it in the bright-line stars, which, on the meteoritic hypothesis, give us the effect of the highest temperature, due to the possibility of end-on collisions without the shielding effects of gaseous envelopes. Kayser expressly states,† "that this series has never been observed before, can perhaps be explained by insufficient temperature in our Geissler tubes and most of the stars."

It seems as if the two series are of the "subordinate" type, and that the principal series is still wanting, because in subordinate series the lines for large values of n lie very near to one another, the principal series on the other hand terminates more in the ultra-violet. It seems, therefore, probable that one or two of the many unknown lines recorded in stellar spectra may belong to the principal series of hydrogen, but that they have not as yet been identified as such.‡

* The two series are as follows:—

Old Series.			*New Series.*		
n.	Computed.	Observed.	n.	Computed.	Observed (means).
6	6563·0	6563·0	5	10128·1	—
8	4861·5	4861·5	7	5413·9	—
10	4340·6	4340·7	9	4543·6	—
12	4101·9	4101·8	11	4201·7	4200·4
14	3970·2	3970·2	13	4027·4	4026·8
16	3889·2	3889·1	15	3925·2	3924·7
18	3835·5	3835·5	17	3859·8	3858·7
20	3798·0	3798·1	19	3815·2	3815·9
			21	3783·4	3783·4

These figures are taken from Professor Pickering's article in 'Astrophysical Journal,' vol. 5, p. 93. See also Kayser's article on p. 95 of the same journal.

† 'Astrophysical Journal,' vol. 5, p. 96.

‡ I find that this conclusion has already been in part arrived at by Kayser ('Astrophysical Journal,' vol. 5, p. 93).

If we are dealing in this case with a single molecule of hydrogen vibrating in a previously unknown way in consequence of a higher temperature, why is it that the molecules of other bodies do not put on similar transcendental vibrations and appear in the same stars so that we shall get new forms of the other chemical elements? The fact that we do not do so is, I claim, an argument in favour of the view that the principal and subordinate series are produced by molecules of different complexities, and that the finer molecules can alone withstand the action of the highest temperatures.

In this way we can easily explain the visibility of the new form of hydrogen in connection only or mainly with the lines of the cleveite and other similar gases (for there is already evidence of the existence of other similar gases) in the hottest stars.

From the admirable work done on such substances as lithium and sodium, which apparently are reduced to their finest atoms at relatively low temperatures, we are bound to consider that when the research includes the complicated spectrum of iron that that also must follow suit; but it is already obvious that a principal and two subordinate series will never do; there will be a very long series of series involved.

Now these series must include both the arc and the enhanced lines, and as these are visible each without the other in stars of different temperatures, in one case associated with the cleveite gases, in another without them, we have another argument in favour of molecular complexity.

Statement of some Opinions.

I am glad to know that attention has recently been drawn to the variations in the appearance of the magnesium lines in the celestial bodies by Dr. Scheiner, of the Potsdam Observatory, who is not apparently acquainted with my work of 1879; he accepts the idea that the variations furnish us with a precise indication of stellar temperature,* and he is now employing it in the work of the observatory.†

* 'Astronomical Spectroscopy,' Frost's Translation, p. viii.

† Dr. Scheiner points out that in the spectra of nearly all stars of Class Ia (Group IV) the line at 4481 "generally appears as a broad line—in some spectra as strong as the hydrogen lines—but its intensity decreases just in proportion as the number of lines in the stellar spectrum increases, so that it is hardly of the average intensity in the solar spectrum, or other spectra of type IIa, and the author is unable to detect it in the spectrum of α Orionis." My prior work, dating from 1879, being probably unknown to Dr. Scheiner, Messrs. Liveing and Dewar are credited with the discovery of the peculiar behaviour of this line in laboratory experiments, and it is added that "the dependence of the line upon the temperature thus readily suggests that the temperature of the absorbing vapours upon the stars

Professors Eder and Valenta thus state the conclusions they have recently arrived at in their study of the changes in the spectrum of mercury :—

"Ferner ist die Erscheinung des ziemlich unvermittelten Aufblitzens des linien-reichsten Spectrums (siehe die Abbildung, fig. 8, der heliographirten Tafel) bei hochgradig gesteigerter Stärke des Flaschenfunkens und gleichzeitigem Erhitzen der Capillare, besonders das Auftauchen zahlreicher neuer Hauptlinien, welche früher nicht oder kaum sichtbar waren, und mancher Doppellinien an Stelle von einfachen Linien, eine derartige, dass sie zu Lockyer's Theorie der Dissociation der Elemente passen würde, wenn man überhaupt die Zerlegbarkeit unserer Elemente in die Discussion ziehen will."*

[Translation :—

"Moreover the appearance of the great brilliancy of the richly lined spectrum with a high tension jar spark, the capillary being heated, and especially the interchange of a great number of new lines which were dim before, and also the change of single lines into double ones; these are such that would harmonise well with Lockyer's theory of dissociation of the elements, if one is prepared to bring into the discussion the possibility of the dissociation of the chemical elements."]

I am glad to be able to quote the following opinion of Mr. Crookes,† to which I attach great weight :—

"Until some fact is shown to be unreconcilable with Mr. Lockyer's views, we consider ourselves perfectly justified in giving them our

of Class IIIa (Group II) is something like that of the electric arc, while that of the stars of Class IIa is higher, and that of stars of Class Ia is at least as high as the temperature of the high-tension spark from a Leyden jar. This view receives striking confirmation in the precisely opposite behaviour of the magnesium line at λ 4352·18. First becoming visible in the spectra of type Ia (Group IV), which have numerous lines, it is strong in the spectra of type IIa (Groups III and V), and increases so as to be one of the strongest lines as we pass towards type IIIa (Group II). Now, as was found by Liveing and Dewar, this line exhibits just the same peculiarities in the laboratory; in the spark spectrum it is hardly recognisable, in the arc spectrum it is very strong."

The association of 4352 with 4481 in the hottest stars, if the stellar line really coincides with the magnesium line 4352, suggests that this is perhaps the last surviving line of the lower temperature spectrum when a very high temperature is reached, but there is nothing in the appearance of this line in the spark spectrum with large jars which would lead us to expect a behaviour similar to that of 4481. It is not impossible, then, that the faint line near 4352 in the hottest stars is not a magnesium line at all.

* 'Denkschriften der kaiserlichen Akademie der Wissenschaften,' Wien, Bd. 61 p. 429, 1894.

† 'Chem. News,' 1879, vol. 39, p. 65.

provisional adhesion, as a working hypothesis to be constantly tested by reference to observed phenomena."

I am anxious to refer here also to the opinion expressed by my friend and colleague, Professor Roberts-Austen, whose researches have mostly been carried on at high temperatures:—

"Mr. Lockyer has, however, since done far more: he has shown that the intense heat of the sun carries the process of molecular simplification much farther; and, if we compare the complicated spectra of the vapours of metals produced by the highest temperatures available here with the very simple spectra of the same metals as they exist in the hottest part of the sun's atmosphere, it is difficult to resist the conclusion that the atom of the chemist has itself been changed. My own belief is that these 'atoms' are changed, and that iron, as it exists in the sun, is not the vapour of iron as we know it upon earth."*

The Basic Lines.

With regard to the basic line part of the inquiry, I think I shall not be going too far in saying that it has been universally rejected, and chiefly on the ground that some lines which appeared coincident at the dispersion I employed appeared double with higher dispersions. I have pointed out in the 'Chemistry of the Sun' (p. 377) that this is not a sufficient answer, but I have left aside this branch of the inquiry for some years in the hope that some chemist would take up the question of spectroscopic impurities out of which it grew.

But it is evident that this basic line point of view, even though it be considered a less direct attack on the problem than others, assumes a much more important and definite position in the light of the new work. I will not go into this question at length now, but will content myself by asking whether one actual demonstration of dissociation will not take a form very like that which the chemist has taken to be a demonstration of the existence of impurities.

§ 3. *Discussion of the Bearing of the Stellar Researches on the Hypothesis.*

Extension of the Field of Investigation.

I now proceed to consider whether the views which I have found necessary to enable me to group together harmoniously and continuously solar phenomena are weakened or strengthened by the study of the new field of investigation opened out by the recent stellar work.

* 'Roy Inst. Proc.,' vol. 13, p. 509, 1892.

In approaching this new part of the subject, it is necessary to proceed with great caution, since the things observed are different. The solar work has consisted in observing different parts of the sun, the star work puts us in presence of the total effects both of radiation and absorption in the case of each body observed.

A Possible Objection.

But *in limine* there is an objection to meet which may possibly be put forward by those unfamiliar with these inquiries.

It is that there is no relation whatever between the sun and stars, that there is no evolution, and that the various spectral differences are due simply to the fact that the relative percentage composition varies from sun to star and from star to star, so that some stars may be composed entirely of the cleveite gases, others of hydrogen, others of calcium, others of iron, others of carbon, and so on.

Those who hold these views and deny that an evolution is going on have to explain the following facts :—

1. While the number of chemical elements known at the present time is about seventy-two, the number of well-marked groups of stars is only seven, and this number includes the nebulæ.

2. It is not sufficient to suppose differences due to a variation of the number of chemical elements present in different stars, as there may also be any number of possible variations in the percentage composition. On this ground an infinite variety of spectra might be expected, but, as already stated, the number of well-marked groups is seven.

3. Each particular kind of star spectrum is always associated with the same degree of stellar temperature as determined by other considerations. With differences of chemical composition, different spectra would occur with equal temperatures.

4. The Sun, Capella, and Arcturus, and other cooling stars, enormously separated in space, contain the same spectral lines with almost identical intensities, so that not only do they contain the same "elements," but they contain them in absolutely identical proportions. The earlier and hotter stages of such stars could not therefore have consisted of different mixtures.

5. All the blood-red stars, which it is generally acknowledged are near the point of extinction, have identical spectra.

6. The facts in this paper already given show that the differences recorded in stellar spectra do not come from a different percentage composition of the elements present, but arise from the action of different temperatures on the same molecules.

Until the above facts are explained I must hold that the argument is complete that we do get the same elements represented by

different spectral lines in different stars when the apparent differences are such as to suggest the objection to which I am now referring.

It is not a question of the absence of elements, but of the absence of certain molecular complexities of each element, which separates the spectrum of the sun from those of the stars of various orders.

The Connexion between Chromospheric and Stellar Phenomena.

The objection having been met, I now proceed to discuss the connexion between the sun and stars, and in order to bridge the gap which separates them, it is necessary to bring forward the very latest solar results as a basis of inquiry.

The chromosphere, which represents that part of the atmosphere with which we have chiefly to do in the light of § 2, is well pourtrayed in the photographs of the Eclipses of 1893 and 1896. So complete is the record, that it is quite sufficient for our present purpose, and is to be the more relied on since it represents it at the same instant of time. I have elsewhere pointed out that Young's list of chromospheric lines may be misleading because it is a summation of results obtained at different times and of different conditions; prominences even may be, and doubtless are, involved. I have already stated the order in which the various substances thin out; the lengths and intensity of the lines are faithfully recorded in the photographs.

An examination of the eclipse photographs shows that the temperature of the most luminous calcium vapour at the sun's limb is not far from that of the electric spark, the blue line being feeble as compared with the H and K lines, which we have seen to be enhanced on passing from the arc to the spark.

Many of the characteristic arc lines of iron appear in the chromospheric spectra as photographed at the two eclipses, and those which I have recently shown to be enhanced in the spark are all present in the chromosphere of 1896, and most of them in that of 1893. In the chromosphere of 1896, the enhanced lines are all of greater intensity than the corresponding Fraunhofer lines, and they are also relatively stronger, as referred to the arc lines, than they are in the experimental spark. Hence, incandescent iron vapour in the chromosphere must be at a temperature at least as high as that of our hottest spark, and certainly higher than that of the iron vapour which is most effective in the production of Fraunhofer lines.

The characteristic spark line of magnesium at wave-length 4481 is not represented in the chromospheric spectra of 1893 and 1896, but other lines which are common to the arc and spark were recorded

in both. Taking the magnesium lines by themselves then, they would suggest a temperature lower than that of the spark, but remembering that 4481 is only a *short* line in the experimental spark, it may be that its absence from the chromosphere is not so much due to low temperature as to the admixture of the magnesium with other vapours.

The intensities of the lines of calcium, iron, and magnesium, therefore, all accord with the view that the temperature of the region of the chromosphere in which these vapours exist is not less than that of the electric spark.

The possible action of heat on the Sun's present Chromosphere.

Having these unimpeachable series of facts to go upon, we have first to consider what would happen if the temperature were raised. We have seen that there are, in fact, in the chromosphere many layers of vapours, that the thickest lines, H and K, are thickest at bottom, and that if we take the lines observed in the spectra of calcium, strontium, iron, magnesium, manganese, &c., the order of thinning out has no known relation to the atomic weights of these substances, while taking the facts as we find them, we are certainly dealing simply with the power of resisting higher temperatures.

Now, it is only possible to consider the results produced by a higher temperature on two hypotheses. The first, the usual one, that the chemical elements are indestructible; the second, that they are not.

On the first hypothesis it is difficult to say what change could take place which would alter the characteristics of the spectrum very widely. We have a complex mixture of the vapours of metallic substances and gases with paramount calcium, hydrogen, and the cleveite gases. Temperature cannot therefore vary the relative intensities of the lines. H and K must always remain predominant, iron must remain because it cannot be destroyed, and the quantity of hydrogen and the cleveite gases present cannot be increased, their lines cannot therefore become more important in the spectrum.

It is also clear that any change of relative density on the usual hypothesis cannot be brought about by an increase of temperature; this, then, cannot alter, it cannot change the relative proportions of chemical substances present in any layer, and therefore the relative intensities of the lines which indicate the existence of the various substances in the different layers.

It may be said that in consequence of a more exalted temperature, the hydrogen and cleveite gases may, for some reason or other, escape from among the metallic vapours, and form an upper special atmosphere of their own, in which, in consequence of its greater

chemical simplicity, the lines of these substances will become more important. But this argument is not philosophical, because we have no right to assume such a change. These gases already exist in the order named in the sun at the temperature of the electric spark, and, in fact, in the sun hydrogen and the cleveite gases give us no traces of their existence at any great height above the chromosphere; the gas that does exist in these elevated regions is one about which we know nothing, so far, terrestrially, and of which no trace has yet been found in the spectrum of the hottest stars.

The only change which we can imagine on the usual hypothesis, as resulting from the increase of temperature, is that with the increase in volume there will be a reduction in density, and all the lines will be equally enfeebled. But this is exactly what does not happen.

If now we turn to the other hypothesis, that, namely, of dissociation, we see at once, in the light of laboratory experiments, that with every considerable increase of temperature in all such masses of vapour and gas as that which now constitutes the solar chromosphere, a fundamental change in the appearance of the spectrum must be brought about, complex molecules would be broken up into simpler ones, and the result of this action would bring new lines into the spectrum, indicating the vibration of the molecules produced. Now let us come to facts. Were the temperature of the chromosphere to be increased, if dissociation takes place at this temperature, the dissociation products must become visible, and we must look for them among those lines which expand at the expense of those which contract and disappear. Is any such experiment as this going on even at this moment? The answer is beyond question. The evidence is complete that the temperature in the reversing layers of α Cygni is higher than that of the reversing layers of the sun. What do we find? Of lines disappearing we have arc lines of iron, some thousands in number, calcium, magnesium, strontium, and so on. Of lines increasing in importance we have the small number representing the enhanced lines of iron, the lines of hydrogen, and some others which we cannot at present associate with the name of any known substance. Here, then, we get a series of phenomena which, on the hypothesis we are discussing, is simply and sufficiently explained by the statement that on passing from the temperature of the sun to α Cygni, among other changes brought about, the complicated line spectrum of iron is giving way to a more simple one consisting of the enhanced lines. Further enquiries show that the other metallic spectra are behaving in the same way. Looking for the lines which increase in importance, while the others are reduced, we find the lines of hydrogen increasing in importance.

The Orion Stars.

Now if we consider another change higher up in the scale of temperature, taking as the lower level α Cygni, at which we have arrived, we have independent evidence that the so-called Orion stars are hotter than such a star as α Cygni.

On proceeding to study the higher dissociating temperature at work in the Orion stars, all the statements made with reference to the changes likely to occur in the spectrum on the non-dissociation hypothesis, strictly apply. We cannot expect any change in the relative intensity of the lines, and the appearance of the spectrum cannot be fundamentally altered.

On the dissociation hypothesis, on the other hand, if we find certain lines indicating certain substances disappearing, and other lines indicating other substances making their appearance for the first time, or if they were visible before, becoming much intensified, we shall have an opportunity of studying the effects of the new dissociating forces at work.

Now is there any change? The facts are that this increase of temperature we are now considering reduces the quantity of iron and calcium present, increases the amount of hydrogen present, and the lines of the cleveite gases now appear.

Associating this with the former result, we get as distinct evidence that an increase in the spectrum of the cleveite gases accompanies the disappearance of the enhanced iron lines as that an increased development of the enhanced iron lines accompanies the decrease of the iron arc lines.

To take iron as an example, for the sake of simplicity, it will be seen then that the actual stellar phenomena might have been predicted up to a certain point, from a consideration of laboratory and solar phenomena. But the stars carry us further than our predictions; we see the gradual increase of hydrogen and cleveite gases. The facts demonstrate that as temperature increases hydrogen increases, and, together with the cleveite gases not obvious before, finally replaces iron which has disappeared.

The Law of Continuity.

I am bound to make this appeal to the law of continuity. The verdict is that, as in all previous human experience, a higher temperature brings about simplifications, and it is not strange that, as our horizon is expanded by new work, we find the same laws in operation. We have, I hold, in these phenomena the work of dissociation carried on before our eyes in the hottest stars, to a point not reached before, and the stars also tell us that this is beyond our

laboratory possibilities, for the highest temperature I have employed only carries us to the heat level of γ Cygni, in which star the cleveite gases, if visible, give only very faint traces. We are thus brought finally face to face with the fact that iron is a compound, into the ultimate formation of which hydrogen, or the cleveite gases, or both, enter, and thus the appearance of D_3 in the chromosphere associated with the enhanced lines of iron is finally explained. The stars included in the present discussion do not enable us to say finally whether hydrogen, excluding the new form, or the cleveite gases, are primordial among those at present known to us, but there is already evidence furnished by β Lyræ and ζ Tauri, that that position will probably have to be accorded to the cleveite gases, dealing with those with which we at present are familiar.

But the discovery of Professor Pickering of the new form of hydrogen, to which allusion has been made, must have a most important bearing upon this conclusion, especially if, as suggested by Kayser and myself, the principal series of hydrogen is still beyond our ken, because this would indicate that even in the hottest stars so far considered, the temperature is not high enough to allow its molecule to exist uncombined.

My son, Dr. W. J. S. Lockyer, is now engaged upon this and allied problems in connection with other series he has discovered in Bellatrix, and the spectra of other stars at the apex of the temperature curve, including the detailed facts with regard to the appearance of the various series of the cleveite gases.

The ultimate Molecules involved.

It follows from the preceding statements that, so far as the power of resisting the effect of temperature is concerned, the ultimate molecules at present within our ken of the chemical elements discussed in this memoir may be arranged provisionally in the following order:—

Uncertain, probably correct order.	Gas X
	He
	H
	Ca
	Mg
	Fe

The ultimate molecules of Ca, Mg, and Fe thus indicated may be considered as representing the vibration of a proto-calcium, proto-magnesium, and proto-ferrum in stars of medium temperature.

The above is put forward with all reserve, for it must be remembered that we are only dealing with stars in our own part of space, and that only a small part of the spectrum has been studied.

Prout's Hypothesis.

This result teaches us that Prout's hypothesis, though at the time it was promulgated it was a magnificent approach to the truth, cannot be true absolutely, for two reasons. (1) Gas X and helium take precedence, in all probability, of hydrogen (excluding the new form) in the evolutionary series; and (2) terrestrial hydrogen, as we know it, is broken up molecularly before it can give us the line spectrum, and it is the hydrogen that gives us the line spectrum that makes its appearance in stars in greater quantity as the enhanced iron lines disappear in consequence of the action of a higher temperature.

Final Result.

The above conclusions, based upon laboratory, solar, and stellar evidence, all tending in the same direction; may be regarded as the result obtained so far in regard to the "celestial dissociation" which I pictured to myself in 1873.

I claim that each step in the work has demonstrated the truth of that hypothesis more and more, and that we can now acknowledge that the phenomena of the inorganic world are dominated by an evolution not less majestic, although much more simple, than that now universally accepted in the case of organic nature.

XII.—General Conclusions. (March 20, 1897.)

1. In a mixture of vapours at a particular temperature, a vapour which is not present in sufficient quantity to show all the lines of its spectrum will be represented by the lines which are longest in its spectrum at the particular temperature in question.

Only some of the short lines in metallic spectra represent the effects of high temperature.

2. Some of the substances which have been investigated, including iron, calcium, and magnesium, have probably a definite spectrum, consisting of a few lines, which can only be completely produced at a temperature higher than any which is at present available in laboratory experiments. The lines constituting the new spectra are those which either only appear in the spark spectrum, or are lengthened in passing from the arc to the spark. Such lines are termed enhanced lines.

3. In the case of iron, calcium, and magnesium, there are four distinct temperature steps which are marked by spectral changes: (*a*) The flame spectrum, (*b*) the arc spectrum, (*c*) the spark spectrum, (*d*) a spectrum consisting solely of those lines which are enhanced in passing from the arc to the spark.

4. The order of temperature of certain stars, as determined from a comparison of the extensions of the continuous spectrum into the violet or ultra-violet, is precisely the same as that which follows from a comparison of the metallic spectra at the four stages of temperature.

5. The variations of the metallic lines furnish the most convenient means of determining relative stellar temperatures, for the reason that photographs with special exposures are unnecessary.

6. Having ascertained the relative temperature of a star in this way, and assuming that all the absorbing vapours are at the same temperature, the presence or absence of any other metallic substance can be determined by looking for the lines which are longest in its spectrum at that temperature. In the case of the hottest stars, the fourth stage spectrum must be the term of comparison.

7. Accepting the new results with regard to the lines enhanced in the spark, several lines in the spectra of the hottest stars, for which no origins could previously be assigned, can now be ascribed to metallic substances at the fourth stage of temperature.

8. The lines of the cleveite gases appear only in the hotter stars, as indicated by the extension of the continuous radiation into the ultra-violet. They increase in intensity with increased temperature in certain stars.

9. The order of stellar temperatures, determined from the *increasing* intensity of the lines of the cleveite gases, is identical with that determined from the *decreasing* intensity of the metallic lines in the case of those stars which show both series of lines.

10. Different substances are spectroscopically visible through different ranges of stellar temperatures. The hydrogen lines are visible in stars ranging in temperature from that of α Orionis to that of Bellatrix, while those of the cleveite gases do not appear below the temperature of α Cygni. The enhanced lines of calcium appear at temperatures as low as α Orionis, and persist, with reduced intensity, to the temperature of Bellatrix; those of iron do not appear at temperatures lower than that of α Cygni, and disappear altogether at the temperature of Bellatrix; while the enhanced line of magnesium appears at the temperature of α Cygni, and remains feebly visible at the temperature of Bellatrix.

11. It follows, then, that the enhanced metallic lines may be absent from a stellar spectrum, either because the temperature is too low or too high.

12. In the case of those stars which previous investigations have shown to be cooling, the metallic line phenomena are inverted. The enhanced lines first become visible, then the arc lines; while the enhanced lines disappear at a certain stage in the process of cooling, the arc lines continue to become stronger.

13. The lines of the cleveite gases show a similar inversion on the downward side of the temperature curve. Strongly represented in the hottest stars, they thin out very rapidly in cooling stars, and disappear before the arc lines have begun to show themselves.

14. Utilising the iron lines as a method of bringing together stars of approximately equal temperature, it is found that at each stage the stars are divisible into two groups, which, in accordance with my previous work, correspond to increasing and decreasing temperatures respectively.

15. As determined in this way, stars of increasing temperature differ from those of decreasing temperature at the same stage of heat: (1) in the greater continuous absorption in the violet or ultra-violet, (2) in the generally greater intensity and breadth of the metallic lines, (3) in the smaller thickness of the hydrogen lines, (4) in the greater thickness of the helium lines at those stages in which they are visible.

16. These differences are all explained on the meteoritic hypothesis.

17. There are stars, near and at the top of the curve, which cannot be arranged in order of temperature by the criteria referred to in 15, for the reason that the iron lines have disappeared, and the lines of hydrogen and cleveite gases show little variation.

18. The arrangement of stars about the top of the curve will depend upon the conditioning of certain lines, at present of unknown origin; the necessary criteria, therefore, require further investigation.

19. The known facts with regard to changes in the line spectrum of an element can be easily explained on the hypothesis of successive dissociations analogous to those observed in the case of undoubted compounds.

20. Similarly, the differences in the lines representative of a metal such as iron in the spectra of sun-spots, prominences, chromosphere, or different stars, are explained by supposing that there are different molecular groupings at each stage of temperature.

21. The change from a continuous spectrum to one consisting of flutings, and afterwards to one of lines, is now acknowledged to be due to the existence of different molecular combinations.

22. The recent investigations of Humphreys and Möhler on the shifts produced in metallic lines, when the vapours are observed at different pressures, confirm my view that the line spectrum of a metal integrates for us the vibrations of several sets of molecules.

23. It is argued that the existence of "series" of lines in the spectra of some chemical elements is another indication of molecular complexity, each series probably representing the vibrations of similar molecules.

24. The behaviour of the magnesium lines in stellar spectra is

ascribed by Dr. Scheiner to differences of temperature, in accordance with my experimental results of 1879.

25. The experiments on the spectrum of mercury which have been made by Eder and Valenta have revealed variations which, according to them, favour the dissociation hypothesis.

26. On various grounds, the view that the differences in stellar spectra represent fundamental differences of chemical composition is untenable. The fact that many stars which are widely separated in space give identical spectra, indicates that they not only contain the same "elements," but that the "elements" exist in the same proportions in all.

27. On the non-dissociation hypothesis, the action of heat on the sun's chromosphere could not produce such a spectrum as that which we know to be associated with hotter stars, since the relative proportions of different vapours could not be changed. The only change which can be imagined to take place on this hypothesis is a reduction of intensity of all the lines due to reduced pressure.

28. On the dissociation hypothesis, increased temperature would bring about fundamental changes in the spectrum due to molecular simplifications, and in this way the effect of an increase of temperature on the sun's chromosphere, as indicated by hotter stars, can be predicted, and receives a simple and sufficient explanation.

29. The disappearance of the enhanced iron lines in the hottest stars, and the simultaneous intensification of the lines of hydrogen, helium, and gas X, bring us face to face with the fact that iron is a compound, into the ultimate formation of which one or all of these gases enters.

30. The ultimate molecules of the chemical elements discussed in the present paper may be provisionally arranged in the following order of resistance to the effects of temperature :—

Gas X, He } Doubtful which.
H
Ca
Mg
Fe

31. Each step in advance which has been made since 1873 has demonstrated more and more that there is really such a "celestial dissociation" going on as that which I then suggested.

In concluding this paper, I am anxious to express my great indebtedness to my assistants. For the photographs of the spectra of stars and the new photographs of the spectra of metals under

various conditions I have to thank Messrs. Baxandall, Shackleton, Butler, and Watson. Mr. Baxandall is responsible for the wave-lengths and intensities of most of the metallic and stellar lines given in the paper, and he has also made the maps which deal more especially with the metallic lines. The maps showing the occurrence of the gaseous lines in stellar spectra have been made by Dr. W. J. S. Lockyer, who has also contributed the part relating to series of lines in spectra. To Mr. Fowler I am indebted for help in the general supervision of the various branches of the research, and for assistance in the preparation of the paper.

Note by Professor SCHUSTER, F.R.S., "On the Chemical Constitution of the Stars."

I could have wished that the classification of star spectra had been the only question submitted to us for discussion. The subject is important and difficult enough in itself; there are many points on which opinions might have been profitably exchanged, and I think we might have arrived at some general consensus of opinion which would have left us more fit to form a sound judgment on the fundamental questions brought forward in the second portion of Professor Lockyer's paper, in which we are directly challenged to accept the dissociation theory in its full generality. Had Mr. Lockyer confined himself to bringing forward his hypothesis as one which is legitimate, consistent, and deserving of attention, many of us would, I think, have agreed that he has made out a good case. But he claims his theory as the only one that can explain the facts, and dismisses as unphilosophical the only alternative which he discusses. I think that in this he has gone too far, and to bring matters to an issue I will put the case for the other side; but before doing so I should like to express my concurrence in the general system of classification adopted in the paper. No classification is likely to prove successful which does not constantly appeal to laboratory experiments, and I do not think Professor Lockyer will have much difficulty in convincing the scientific world that he has made a very material advance by his investigations of the "high temperature" or "enhanced" lines of the metals.

"The only change," according to Professor Lockyer, "which we can imagine on the usual hypothesis as resulting from an increase of temperature, is that with the increase in volume there will be a reduction in density, and all the lines will be equally enfeebled." With this remark I cannot agree. The main fact to be explained is the gradual displacement of hydrogen, which is predominant in the hottest stars, by calcium, iron, and other metals. There are in my

opinion several causes at work which might produce that effect. A glowing mass of gas may be either in thermal or in convective equilibrium, and the spectroscopic appearance in the two cases will be profoundly different. In reality an intermediate state probably is arrived at, but there is good evidence to show that the state of convective equilibrium is more nearly approached in our sun than in the hydrogen stars. We know as a fact that there are powerful convection currents near the sun's surface. There is in consequence an approach to a uniform distribution of matter and enormous differences of temperature in layers which are comparatively close together. Those who have not given much attention to this subject will hardly realise the differences of temperature brought about by convection currents. On the surface of the sun the temperature gradient produced by convection currents would be equal to 20,000° for each 100 km. difference in level, so that an angular distance of one second of arc would correspond to a difference of 100,000°. Radiation and condensation will diminish this gradient, but that it is very large is sufficiently proved by the spectroscopic evidence. Thus, according to the results of Messrs. Jewell, Mohler, and Humphreys,* the pressure in the reversing layer for hot calcium giving the H and K lines is about 6 atmospheres, while that for the cooler calcium vapour is about 3 atmospheres. With a gravitational constant 27 times as large as that of our earth, a difference of 3 atmospheres can only mean a comparatively small difference in level. While, then, in the sun we must admit a more or less effectual stirring up of the constituents together with an accompanying rapid temperature gradient, the evidence is just the other way in the case of stars like γ Lyræ. The spectrum of that star, according to Professor Lockyer, contains only the high-temperature lines of iron. This means not only that the reversing layer is very hot, but also that there are no rapid changes of temperature at different levels. It is impossible to imagine this hot layer of gas ending abruptly, it must be surrounded by cooler matter, which cannot be iron, as the low-temperature lines of iron do not appear. In such a star there cannot be an effectual mixing up of the constituents, and hence the layers of gas will arrange themselves according to the laws of diffusion. It would follow that hydrogen being a lighter gas than iron will be chiefly represented in the cooler and outer layers, while iron will be found more particularly in the inner and hotter parts. The relative proportion of different elements in different layers will be regulated partly by their density, but to a great extent also by the total quantities present in the star; for the different gases will not float on each other as liquids might, but the density of each gas will increase steadily from the surface to the centre. The chief difference, according to this view,

* 'Astrophysical Journal,' vol. 3, p. 138.

between a hydrogen and a solar star lies in the more or less effectual mixing up of the constituents. If we could introduce a stirrer into γ Lyræ there can be no doubt whatever that the low-temperature lines of iron would make their appearance, while, on the other hand, if we could stop all convection currents on the surface of the sun the hydrogen which now lies under the photosphere would gradually diffuse out and give greater prominence to its characteristic absorption lines.

In the face of the direct evidence of the absence of convection currents in the hotter stars, it is not necessary for the purpose of my argument to discuss why this is the case, but it can be seen that diminished gravity, diminished density, and consequently increased viscosity, will contribute to the effect, while effectual radiation will, owing to the smaller density, take place more evenly through a thicker layer of the envelope, so that the principal cause of convection currents will also be much diminished. The explanation I have given does not differ materially from that brought forward by Dr. Huggins in his address as president of the British Association at Swansea. I do not say that difficulties may not be raised against it, but difficulties may be raised also against the theory of dissociation, and there is especially one question which Professor Lockyer must be prepared to answer. Amongst the heavier metals, tellurium, antimony, mercury, are not represented in the sun, but they are found in Aldebaran. To be consistent, we must, if we adopt the theory of dissociation, assert that these metals are decomposed in the sun. But if I understand Professor Lockyer right, he believes that with our strongest sparks we can exceed the state of dissociation which exists in the reversing layer of the sun. Take such a strong spark then from a pole of mercury, do you get lines of helium, or of calcium, or of hydrogen? This seems to me to be almost a crucial experiment. Possibly, of course, we should get high-temperature lines not hitherto looked for, but present in the sun. If so, the objection would fall to the ground, but if this is not the case, and if mercury at a high temperature refuses to be dissociated into simpler elements, a most serious objection to the theory would have to be answered.

With regard to the division of stars into those of rising and those of falling temperature, some of Professor Lockyer's remarks lead me to think that he has not followed the recent development of the nebular theory. To him the nebular theory is still that of Kant and Laplace, while Lane, Lord Kelvin, and Ritter have applied the dynamical theory of heat to the treatment of gaseous masses, contracting under their own gravitation, and have clearly shown that a *radiating* and *contracting* mass is not necessarily a *cooling* mass; on the contrary the interior of our sun is almost certainly rising in temperature at the present moment. Ritter's work is of great importance.

He showed in the year* 1883 that the total amount of light emitted by a star will, on the nebular hypothesis, first rise and then fall, and he then suggested that the stars of Secchi's Class IV were falling in temperature, while those of Secchi's Class III were rising. This is the same distinction which Lockyer afterwards based on his meteoric theory, and this division is therefore not dependent on the truth of that theory.

While I think that we shall all admit that different stars are in different stages of development, and that hydrogen stars will ultimately approach more nearly to the state of our sun, it would be unwise to push the argument of uniformity too far, and to say that every star will pass exactly through the same stages. Ritter, who is favourably inclined to the dissociation hypothesis,† gives good reason to believe that the sun's surface was never much hotter than it is now, and that the higher temperature of hydrogen stars is connected with their greater masses. It is, in fact, impossible to admit that the process of development should be quite independent of the total mass of the star. It may be urged that Arcturus must have a mass much larger than that of our sun, and its spectrum, according to Professor Lockyer, is identical with that of the sun. But I suppose that that statement only refers to the blue and violet region, for according to Dr. Huggins, to whose early stellar photographs we owe so much, the spectrum of Arcturus in the ultra-violet approaches that of Sirius.

Further, though I am ready to agree with the general statement that the chemical elements in the stars are the same as those in the sun, and probably in not widely different proportions, I am not convinced that the difference in the spectra of stars, which Mr. Lockyer ascribes to ascending and descending temperatures, are not due to a real difference of constitution.

We have no reason to believe that the nebulæ of the present day resemble our sun's ancestor. Some of the stars which are now in an early stage of development may be forming through the condensation of matter which has been left over by others; and it would not be surprising if the youngest star did not agree in constitution with its aged companions.

I now pass on to say a few words on Mr. Lockyer's final conclusions:—Most of us are convinced in our innermost hearts that matter is ultimately of one kind, whatever ideas we may have formed as to the nature of the primordial substance. That opinion is not under discussion. The question is not whether we believe in the unity of matter, but whether a direct proof of it can be derived from the spectroscopic evidence of stars; for that is what Professor Lockyer claims. It must be observed that even according to him,

* 'Wied. Annalen,' vol. 20, p. 156.

† 'Wied. Annalen,' vol. 20, p. 152.

we do not witness in these stars the original formation of our elements; for his own theory supposes the stars to be formed from condensing matter originally cool, in which the metals, as we know them, are already present. What we see in these stars, as I understand his theory, is a case of atavism, a temporary relapse of our elements into their original state. That may be so. It is in my opinion as I have already stated, a perfectly legitimate hypothesis, one that at present has not been disproved, and for which, as well as against which, a good deal might be said, but I cannot admit the claim, that it is demonstrated. Nor do I believe that it will be generally accepted until some direct experimental evidence is obtained. The resources of our laboratories are not yet exhausted; and we have not arrived at the limit to which we can push the intensity of our electric sparks. If Mr. Lockyer is right we must look forward to finding some trace of helium, or calcium, or hydrogen in the discharge taken from iron poles. When that is done, and not till then, will this theory be considered as proved.

Additional Remarks by Mr. J. Norman Lockyer.

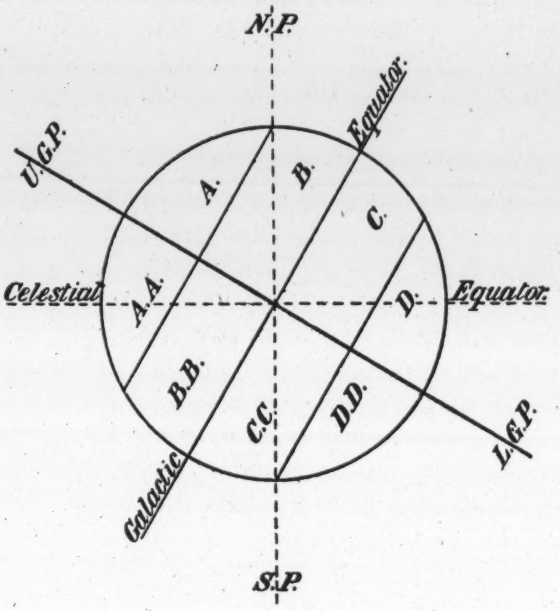
Mr. Lockyer, in his reply, pointed out that it was not open to him to discuss the question of classification. He had been asked to prepare a paper on another subject, for the proper consideration of which the question of classification had to be referred to. If Professor Schuster, as he says he does, concurs generally in Mr. Lockyer's scheme of classification, he must acknowledge that there must be a very great difference in the amount of "stirring" on the two sides of the temperature curve, and that powerful convection currents must exist in the hotter stars in which the temperature is rising; and yet as a matter of fact the line absorption is very similar in both arms of the curve.

Mr. Lockyer has nowhere claimed that his hypothesis is the only one which can explain the facts, he only claims that it is simple and sufficient.

Note by Mr. Frank McClean, F.R.S., "On Comparative Photographic Spectra of Stars to the 3½ Magnitude."

The 160 photographs which accompany the paper* include, with insignificant exceptions, all stars equal to and brighter than the 3½ magnitude contained in five out of eight equal areas, into which the celestial sphere has been divided.

* The paper to which reference is made, and on which Mr. McClean's remarks were based, was read subsequently, at the meeting of April 8.



The diagram shows the position of the eight areas consisting of—

Two upper galactic polar regions, viz.,			A	and	AA.
,, ,, ,,	zones	,,	B	,,	BB.
Two lower galactic zones		,,	C	,,	CC.
,, ,, ,,	polar regions	,,	D	,,	DD.

The galactic zones extend to 30° from the galactic plane.

The object of the division into equal areas is to bring out roughly any differences of distribution of the different types of spectra in relation to the galactic plane.

The areas A, B, C, D, and AA are included in the present series of photographs. Of these the first four constitute a complete hemisphere, symmetrically divided by the galactic plane, and the deductions as to distribution have been made from them.

The stellar spectra have been arranged in series, and classed separately in the respective areas to which they belong. It appears at once that Secchi's Type I requires further subdivision into distinct classes. To effect this, a series of divisions in parallel to Secchi's types have been adopted, in which Divisions I, II, and III correspond to Type I, and Divisions IV, V, and VI to Types II, III, and IV respectively.

Classified tables of the stellar spectra are given, and previous classifications by Secchi, Vogel, Pickering, and Lockyer are indicated in separate columns.

Three lines of evidence are adduced to show that the helium stars

of Division I are in the first stage of stellar development from the gaseous nebulæ. Firstly, the correspondence of their absorption spectra with the bright-line spectrum of the gaseous nebulæ, as shown by Campbell and others, is pointed out. Secondly, it is shown that the photographic spectra of the groups of stars involved in, or connected with, the nebulæ in the Pleiades and the great nebula in Orion are helium spectra of Division I. Thirdly, it is shown that the helium stars of Division I, and the gaseous nebulæ are similarly distributed in the galactic zones.

The special characteristics of Divisions I, II, and III are discriminated as follows:—Bright-line stars are classed in accordance with their absorption spectra.

Division I.—The Orion stars, or the helium type. The general characteristics are the spectra of cleveite gas and hydrogen. The K line of calcium is usually wanting.

Subdivision (*a*).—There are, in addition, special lines, the source of which is unknown. The possibility is suggested of these being due to oxygen. Thalén's spark spectrum of oxygen is collated with the stellar spectra.

Subdivision (*b*).—There are other special lines attributed to calcium, barium, and magnesium. These lines are persistent in subsequent divisions, and mark the order of transition from Division I to Division II. The helium lines gradually disappear.

Division II.—Sirian stars, or the hydrogen type. The characteristics are:—Helium wanting—Very strong hydrogen—The K line usually sharply defined—A delicate line spectrum due to calcium, titanium, and faintly to iron—α Cygni belongs to this type, and shows the characteristic line spectrum exceptionally distinct—The ultra-violet spectrum is very strong.

Division III. — Procyon stars, or the hydrogen-iron type. Characteristics:—Strong hydrogen—Strong K—Usually a full iron spectrum—The violet spectrum still strong, and the red weak—The iron spectrum is collated with the photograph. This division completes the requisite subdivision of Secchi's Type I.

Divisions IV, V, and VI are equivalent to Secchi's Types II, III, and IV. Division IV is the solar type, and on account of our knowledge of the solar spectrum it forms the basis of sidereal spectroscopy.

The block of space explored by means of the photographs is n diameter about seven times the mean distance of first magnitude stars from the sun. This is equivalent to about 255 light-years.

The following is the table of distribution of the stellar spectra here given. The distribution of the gaseous nebulæ, planetary and extended, has been added.

Table of Distribution of Gaseous Nebulæ and of Stellar Types.
Stars to the 3½ Magnitude.

	A.	B.	C.	D.	Total.	AA.	BB.	CC.	DD.	Total.
Planetary nebulæ	2	3	8	2	(15)	2	7	3	0	(12)
Extended „	1	4	8	4	(17)	1	4	3	1	(9)
Total gaseous nebulæ.	3	7	16	6	(32)	3	11	6	1	(21)
Stellar Types.										
Division I	3	6	17	3	(29)	6				
„ II	10	7	0	3	(20)	3				
„ III	7	8	8	4	(27)	9				
„ IV	14	8	9	13	(44)	9				
„ V	1	2	4	3	(10)	3				
Total stellar spectra	35	31	38	26	(130)	30				

Note.—The gaseous nebulæ are those given in the table in Frost's edition of Scheiner's 'Astronomical Spectroscopy.'

We gather from this table that as the stellar types of spectra become more advanced they are found to be more evenly distributed in space. The idea is suggested that stars of the solar type—Division IV—started their career as helium stars of Division I, before the condensation of the galaxy.

The Procyon stars of Division III followed in the same way after the galaxy was formed. The paucity of stars of this type in the lower polar region, coupled with their even distribution in the other areas, suggests the idea that the sun itself is situated near the lower boundary of the galaxy.

The distribution of the Sirian stars, Division II, is anomalous, and further information is required as to their distribution in the southern areas.

The Orion stars—Division I—are almost confined to the galactic zones. It has been already conjectured that they are still in the first stage of stellar development from the gaseous nebulæ.

It has been throughout assumed that the successive types or divisions are merely the manifestations of the successive physical states, through which every star naturally passes in the course of its career.

"Experiments on the Flame Spectrum of Carbon Monoxide."

By W. N. HARTLEY, F.R.S., Royal College of Science, Dublin. Received March 6,—Read March 18, 1897.

In the investigation of flame spectra at high temperatures, 'Phil. Trans.,' A, vol. 185, p. 676, Part I, 1895, it was found that carbon monoxide when burnt with oxygen gave faint lines or bands in its spectrum coincident with edges of some of the carbon bands which have been observed in the flame of coal-gas. The experiments were made simply with the view of ascertaining what might be usually seen in a carbon monoxide flame, and looked for in a flame from a Bessemer converter. The spectrum was photographed with a fairly wide slit, and the exposure was of one hour's duration. It was composed almost entirely of a continuous band of rays, but there were very feeble indications of lines or the edges of bands which, after careful inspection, being barely visible, were just capable of being measured. Their positions lay between wave-lengths 5945 and 4249. Other faint bands were seen at 6337, 6172, 4224, and 4183. These all lie within the region of coloured rays and apparently belong to carbon, but they were by no means considered an important feature in the spectrum.

The exact nature of the carbon monoxide spectrum bears upon the researches of Professor Smithells ('Trans. Chem. Soc.,' 1894, p. 603), so that it seemed desirable to decide whether the lines observed were really due to carbon monoxide gas, or to some impurity which gained admission to the material burned, or to the flame produced by its combustion. A little consideration showed that possibly carbonaceous dust, in other terms, organic matter floating in the air, might be concerned in the production of the feeble carbon bands, as the exposure was a long one. Again, as the gas was conveyed from the gas-holders and also from the oxygen cylinders through tubes of india-rubber, there was some amount of uncertainty as to whether the tubes had not contributed to the gases some minute quantity of a volatile hydrocarbon. To obviate any such complication, a further series of photographs was taken with a spectroscope slit of narrower dimensions to obtain better definition.

The gases were conveyed through patent flexible steel tubing which had not been used for any other purpose. The flame was produced by burning the carbon monoxide by means of a Deville blow-pipe fitted with a polished platinum nozzle. Oxygen was conveyed into its interior by a polished platinum jet. The image of the flame was projected on to the slit, and the photographic plates were exposed from thirty-five to forty minutes. One part of the spectrum

was photographed more strongly than the rest, the slit being wider, so that any feeble lines not easily seen under the usual conditions might be made apparent. Two spectra were photographed, but one was rather too weak to justify conclusions drawn from its appearance.

Presence of the Water-vapour Spectrum.

The second spectrum was sufficiently dense, and its examination disclosed the curious fact that it contained two groups of the water-vapour lines (Liveing and Dewar, 'Phil. Trans.,' vol. 179, p. 27). The strongest group in the carbon monoxide spectrum belonged to Section VI, wave-lengths 3428, 2472, 3548, and 3933, the last line corresponds in position with the solar line K. It is stated that the lines in this section are weaker than those in Section V of the oxy-hydrogen flame, which lie between 3063 and 3203·5. Now, although these latter lines were photographed in the carbon monoxide they were of much feebler intensity than those in Section VI.

The lines were identified by comparing them with those from the flame of the oxyhydrogen blow-pipe photographed with the same instrument, with which they were found to coincide. That both are obtained by the same process of combustion, but by gases from different sources and with groups of lines with different relative intensities, was thus established.

There were no other lines visible, only a continuous band of rays.

Possible Origin of the Water-vapour Lines in the Carbon Monoxide Spectrum.

As to the possible origin of these two groups of lines, it was at once conceivable that the water-vapour present in the carbon monoxide was the cause of their appearance.

Another spectrum was then photographed, using on this occasion a four-prism instrument, but there is, unfortunately, no means of obtaining on one plate both groups of lines including those more refrangible than 3502.

The gas was dried by passing it through pumice soaked in oil of vitriol. A continuous spectrum was visible, with very slight indications of some of the less refrangible of the water-vapour lines. A further trial was made of the dried gas, which in this case was burnt from the interior or oxygen jet of the Deville blow-pipe, the oxygen passing through the platinum nozzle of the outer tube. A steady brilliant flame burnt for fifty minutes. In this instance the two groups of the water-vapour lines were as distinct as before, with a continuous band of diffused rays, but no other lines or bands were seen. It may be remarked that damp weather prevailed during these

experiments, and though dry oxygen surrounded the dry carbon monoxide gas, yet the moisture of the atmosphere may have contributed the water-vapour.

In the original photograph of the carbon monoxide there were no water-vapour lines apparent in the spectrum. Their invisibility may be accounted for by a want of definition, owing to their being photographed with a more widely open slit. That the relative intensities of the two groups of lines are not the same as in groups V and VI of Messrs. Liveing and Dewar's photographs, furnishes grounds for believing that they belong to the spectra of two different substances. It is not improbable that one of these substances is an oxide of nitrogen, for it has long been known that nitric acid is produced in minute quantity by burning hydrogen, and there is reason to believe that it is also formed on the outside of a carbon monoxide flame, where a greenish-yellow tinge is observed.

It has been suggested to me by Professor Smithells that the existence of the water-vapour spectrum, in the circumstances just described, may be a confirmation of the observation made by Dixon, that carbon monoxide when dry will not burn in dry air ('Phil. Trans.,' 1884, Part II, p. 629; see also Brereton Baker, 'Trans. Chem. Soc.,' 1894, p. 611). Smithells states, however, that it will burn if the carbon monoxide is heated ('Trans. Chem. Soc.,' 1894, p. 610). This latter fact renders it possible for carbon monoxide to burn and yet not show the water-vapour spectrum, since, though a damp atmosphere may cause the gas to ignite, the flame will heat the jet and thus the gas subsequently will be heated and burned, although both it and the oxygen are dry. It may also serve to account for the original carbon monoxide spectrum not exhibiting the water-vapour lines.

The water-vapour spectrum was always more feeble in the photographs taken from the carbon monoxide flame under an exposure of from thirty-five to fifty minutes than in the oxyhydrogen flame under an exposure of only two minutes. This indicates the small proportion of the water or other substance present.

The result of these experiments shows that the spectrum of carbon monoxide consists entirely of a continuous spectrum decreasing in intensity towards the more refrangible part of the ultra-violet, about wave-length 3000. No carbon bands or any lines or edges of bands were photographed which could not be accounted for as due to other substances than carbon monoxide.

April 1, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

Professor Johannes Wislicenus was balloted for and elected a Foreign Member of the Society.

The CROONIAN LECTURE, "The Mammalian Spinal Cord as an Organ of Reflex Action," was delivered by Dr. C. S. SHERRINGTON, F.R.S.

CROONIAN LECTURE.—"The Mammalian Spinal Cord as an Organ of Reflex Action." By C. S. SHERRINGTON, M.A., M.D., F.R.S., Holt Professor of Physiology, University College, Liverpool.

(Abstract.)

The channels of connection between spinal nerve-centres.

Long spinal reflex paths and short spinal reflex paths.

The First Law of Pflüger: examples of it and exceptions to it; relation between it and microscopical features of the cord.

The Second Law of Pflüger: examples of it and exceptions to it; the crossed knee jerk.

The Third Law of Pflüger: examples of it and exceptions to it.

The Fourth Law of Pflüger: examples of it and exceptions to it; influence of semisection upon the conductions.

The contradistinction drawn between spinal reflexes and cortical reactions.

The interpretation of purposive character of spinal reflexes.

Relation of the reflexes to progression.

The solidarity of the isolated spinal cord as a "reflex" organ.

Some characters of spinal reflex movements.

Changes induced in reflex actions by shifting the site of transection from upper bulbar to infra bulbar situations.

The correlation augmentations with inhibitions.

Influence of the afferent spinal root on willed movements, on cortical reactions, on tonus, on rigor mortis, on knee jerk. Areal induction (simultaneous contrast) as a spinal phenomenon. Influence of afferent roots on spasm, on spinal reflexes.

Is the efficiency of the afferent root as an excitor of reflexes influenced by distal severance of it?

Double conduction in the central nervous system; information as to the connections of the long ascending fibres of the dorsal columns.

The locus of incidence of areal induction in the synapse.

A particular case in regard to the relation between afferent and efferent neurons is offered by the eye-muscles. The source of tonus of the eye-muscles.

The importance of afferent roots to muscular activity. The distinction between anæsthesia and apæsthesia.

The question of the independent excitability of the motor neuron.

April 8, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Production of X Rays of different Penetrative Values." By A. A. C. SWINTON. Communicated by LORD KELVIN, F.R.S.

II. "Photographic Spectra of Stars to the $3\frac{1}{2}$ Magnitude." By F. MCCLEAN, F.R.S.

III. "Condensation of Water Vapour in the Presence of Dust-free Air and other Gases." By C. T. R. WILSON, B.Sc. (Vict.), M.A. (Cantab.), of Sydney Sussex College, Clerk-Maxwell Student in the University of Cambridge. Communicated by Professor J. J. THOMSON, F.R.S.

IV. "Double (Antidrome) Conduction in the Central Nervous System." By C. S. SHERRINGTON, M.A., M.D., F.R.S., Holt Professor of Physiology, University College, Liverpool.

V. "Further Note on the Sensory Nerves of Muscles." By C. S. SHERRINGTON, M.A., M.D., F.R.S., Holt Professor of Physiology in University College, Liverpool.

VI. "On the Breaking-up of Fat in the Alimentary Canal under Normal Circumstances and in the Absence of the Pancreas." By VAUGHAN HARLEY, M.D., M.R.C.P., Professor of Pathological Chemistry, University College, London. Communicated by Professor HORSLEY, F.R.S.

VII. "On the Application of Harmonic Analysis to the Dynamical Theory of the Tides. Part I. On Laplace's 'Oscillations of the First Species,' and on the Dynamics of Ocean Currents." By S. S. Hough, M.A., Fellow of St. John's College and Isaac Newton Student in the University of Cambridge. Communicated by Professor G. H. Darwin, F.R.S.

VIII. "On Boomerangs." By G. T. Walker, M.A., B.Sc., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. Thomson, F.R.S.

IX. "Cathode and Lenard Rays." By J. A. McClelland, M.A., Junior Fellow R.U.I. Communicated by Professor J. J. Thomson, F.R.S.

X. "On the Structure and Affinities of Fossil Plants from the Palæozoic Rocks.—On *Cheirostrobus*, a new Type of Fossil Cone from the Lower Carboniferous Strata (Calciferous Sandstone Series)." By Dr. D. H. Scott, F.R.S.

"The Production of X Rays of Different Penetrative Values."

By A. A. C. Swinton. Communicated by Lord Kelvin, F.R.S. Received March 24,—Read April 8, 1897.

As is well known, if the X rays coming from an ordinary Crookes tube of the Jackson focus type be observed with a fluorescent screen during the process of exhaustion, the penetrative value of the rays is found to change as the exhaustion proceeds.

First of all, at less than a certain degree of vacuum, no X rays are produced. Next, as the vacuum is increased, X rays commence to show themselves, but of a quality that will do little more than penetrate the backing of the screen. As the vacuum is further increased, the rays become more penetrative, and show the shadow of the bones in the hand. As exhaustion proceeds further, a point is reached when the flesh of the hand seems to be almost completely transparent, while the bones are almost entirely opaque. At higher vacua than this, the bones becoming more and more transparent, the contrast between bones and flesh becomes less and less, till at length, at the very highest vacuum at which the discharge will pass, the bones scarcely show at all, owing to their having become nearly as transparent as the flesh, while the whole hand throws but a very faint shadow on the screen.

Similarly, it is found that, at any given degree of vacuum, the penetrative value of the X rays is increased by increasing the power

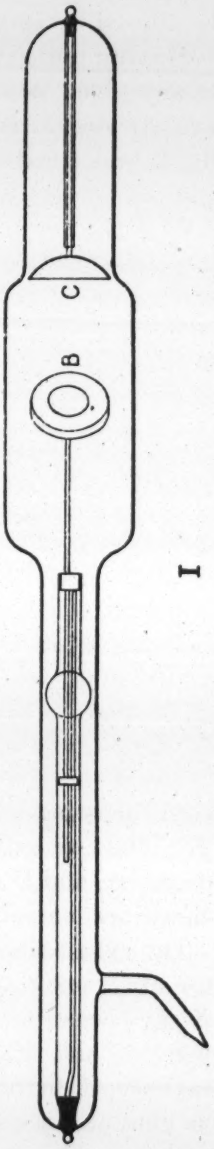
of the Ruhmkorff coil, and thereby increasing the difference of the electrical potential between the cathode and the anode portions of the tube, as measured by the length of the alternative spark in air.

Again, similar results are obtained without alteration to the vacuum or to the power of the Ruhmkorff coil by varying the resistance of the tube by means of a magnetic field.*

In this case the gradual strengthening of the magnetic field produces a gradual decrease in the resistance of the tube and of the difference of the electrical potential between the cathode and anode, and, at the same time, causes a gradual diminution of the penetrative value of the X rays.

I have further found that it is possible to vary the penetrative value of the X rays produced in a focus tube by simply altering the distance between the cathode and the anti-cathode.

For this experiment the tube, fig. 1, was constructed, in which the anti-cathode B of aluminium, faced with platinum, is connected to the anode terminal by a sliding steel rod, so that it could be moved along the axis of the tube, and the distance between cathode C and centre of the anti-cathode B varied from 1 to 3 inches, the anti-cathode being always outside the focus of the cathode. The tube was exhausted to a degree that gave a maximum contrast between the bones and flesh of the hand when the anti-cathode was midway between its two extreme positions, and was then sealed off, so that the vacuum was not varied during subsequent experiments. Upon the anti-cathode being approached to the cathode, the X rays immediately became of a more penetrative value, just as though the vacuum had been increased, while, at the same time, the potential difference, as measured by the alternative spark, was found to have risen. Again, when the anti-cathode was moved in the opposite direction and placed at a greater distance from the cathode, the potential difference fell, and the X rays became less penetrative and similar to those produced at a lower vacuum. In this way, without varying the vacuum, the pene-



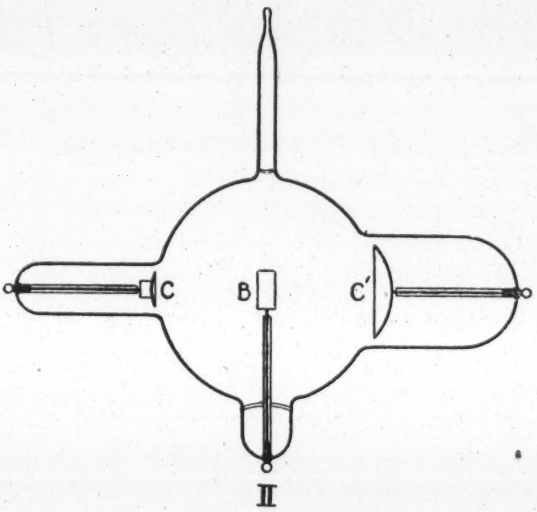
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* See "The Effects of a Strong Magnetic Field upon Electric Discharges *in Vacuo*," 'Roy. Soc. Proc.,' vol. 60, p. 179, and note in 'Nature' for July 9, 1896, p. 239.

trative value of the X rays could be increased or decreased as desired within the limits of the focus on the one hand and the travel of the anti-cathode on the other.

Again, I have found that the penetrative value of the X rays can be altered by employing cathodes of different diameters.

Having noticed that focus tubes with small cathodes give X rays of any given penetrative value at a much lower exhaustion than do exactly similar tubes having cathodes of larger diameter, the tube, fig. 2, was constructed, in which there are two cathodes, C and C′,



II

both focussing upon opposite sides of the same platinum anti-cathode, B. The two cathodes were of dissimilar diameter, C being 0·375 in. diameter, and C′ 1·125 in. diameter. Both had the same radius of curvature, namely, 0·75 in.

On exhausting this tube with the smaller cathode, C, in use, and the other not connected, a degree of vacuum was soon reached when X rays began to be produced plentifully. Pumping was then suspended, and the negative connection changed from the smaller cathode, C, to the larger, C′. Under these conditions, no X rays could be obtained, the vacuum being evidently much too low for the larger cathode. On continuing the exhaustion to the point when, with the larger cathode in operation, X rays were produced, but of a low penetrative value, the negative connection was removed from the larger cathode, and the smaller cathode put into use. Immediately it was found that the penetrative value of the X rays had increased to an extent that could only be obtained with the larger cathode with a very much higher vacuum.

Throughout the experiments with this tube it was further found

that the potential difference, as measured by the alternative spark in air, was much greater when the small cathode was in use than with the larger cathode.

The penetrative value of the X rays produced by any given tube appears, therefore, to be dependent upon several conditions.

1. The penetrative value is higher for a high vacuum than for a low vacuum.

2. It is higher when the electrical power applied is great than when it is small.

3. It is higher when the resistance of the tube is great than when this resistance is reduced by magnetic means.

4. It is higher when the distance between the cathode and anti-cathode is small than when the distance is great.

5. It is higher when the cathode itself is small than when it is large.

6. It is higher when, as a consequence of one or more of the above, the potential difference between the cathode and the anode portion of the tube, and consequently the electrical excitation of the cathode, is great than when it is small.

On the assumption that the cathode rays consist of negatively charged molecules that are repelled from the similarly electrified cathode with an initial velocity that depends upon the degree of electrical excitation of the cathode, the above conditions are those that would conduce to a high average velocity of the molecules at the moment at which they strike upon the anti-cathode, and, at the same time, to a high average difference of potential between the travelling molecules and the anti-cathode at the moment of impact.

At high exhaustions, not only is the electrical excitation more, and the initial velocity of the molecules consequently greater, but, owing to the smaller number of collisions with the other molecules of residual gas in the tube, the average velocity of the molecules and the amount of their negative charge have suffered less diminution by the time they reach the anti-cathode than in the case of lower exhaustions.

Similarly, when more electrical power is employed, the electrical excitation and the initial velocity of the molecules is increased, while the employment of a magnetic field reduces the resistance and the electrical excitation, and consequently reduces the initial velocity and negative charge of the molecules.

Again, when the anti-cathode is near to the cathode, the moving molecules having a less distance to travel before they reach the anti-cathode, have by that time lost less of their initial velocity and charge by collisions than when the anti-cathode and cathode are further apart.

And, lastly, not only does a small cathode become charged to a

higher electrical potential than a large one, and consequently impart a higher initial velocity and charge to the molecules, but with a small cathode the travelling molecules are more compactly arranged than with a large cathode, and consequently are not likely to make so many collisions and lose so much in velocity or electrical charge during their transit.

It would, therefore, appear that, whatever the precise cause, the penetrative value of the X rays produced under any given set of conditions is dependent upon the average velocity of the molecules and the difference of potential between them and the anti-cathode at the moment of impact, being higher the higher the velocity and the greater the potential difference.

Further, since the excitation of the cathode is not uniform, but varying, so that different molecules have different initial velocities and charges imparted to them, and since some of the molecules will make fewer collisions than others, and some molecules will thus strike the anti-cathode at higher velocities and in a more highly charged state than others, the same hypothesis will account for X rays being more or less heterogeneous under all conditions.

Finally, it appears that the penetrative value, as distinct from the quantity, of X rays is independent of the material of which the anti-cathode surface is made. Experiments with a tube in which the anti-cathode was made partly of platinum and partly of aluminium, and so arranged that by inclining the tube the anti-cathode could be moved, and either the platinum or the aluminium part could be brought into use, show that, even with metals having such very dissimilar atomic weights, the penetrative value of the X rays produced was the same, though the quantity of the rays as measured by photographic action or by the brightness of a screen of barium platino-cyanide was distinctly greater with the platinum. Further experiments with other tubes fitted with anti-cathodes of aluminium, iron, copper, silver, and platinum confirm these results. The metals of high atomic weight form the most efficient anti-cathodes, and give a larger quantity of X rays, though the difference is not so great as might, perhaps, be expected. All, however, appear to give X rays of the same penetrative value under similar conditions.

I am indebted to the assistance of Mr. J. C. M. Stanton and of Mr. H. L. Tyson Wolff for the exhaustion and manufacture of the various tubes with which the above experiments have been made.

"Cathode and Lenard Rays." By J. A. McClelland, M.A., Junior Fellow R.U.I. Communicated by Professor J. J. Thomson, F.R.S. Received March 15,—Read April 8, 1897.

(From the Cavendish Laboratory.)

The close connexion between the cathode rays and the more recently discovered Röntgen rays gives a new interest to the study of the properties of the former, although, indeed, the two widely different views that are held as to the nature of these rays render such an impetus unnecessary.

One theory holds that the cathode rays are a stream of material particles shot off from the negative electrode, and carrying with them a negative charge, while the other theory ascribes the rays to a phenomenon in the ether. Both theories have received a large amount of support.

Hertz* found that thin films of gold-leaf, when placed in the path of the rays were sufficiently transparent to allow a slight phosphorescence to be produced behind the films.

This at first sight does not appear consistent with the view that the cathode rays consist of a stream of charged particles, and that the phosphorescence is produced by the rapid approach of these particles up to the surface of the phosphorescing substance. Even the thinnest films of gold-leaf could not allow the passage through them of such material particles.

But it has been suggested by Professor J. J. Thomson† that under the action of the cathode rays the thin film of gold-leaf itself acts as a cathode, and thus from its further surface we have rays given off similar to those given off by the primary cathode.

If this be the true explanation of the phenomenon we would expect these secondary cathode rays to be identical in their properties with the primary cathode rays which produce them.

Recently Lenard,‡ in a very valuable series of experiments, has studied the secondary rays which are produced, and which are usually called by his name, when the cathode rays fall upon thin films of aluminium, and the results he obtains appear to be explicable by the above-mentioned suggestion of Professor J. J. Thomson.

The experiments described in this paper have to do, partly with these Lenard rays, and partly with the cathode rays themselves. In the first place experiments were devised to test whether the rays produced by the incidence of the cathode rays on thin films carried a negative charge as the cathode rays do.

* 'Wied. Ann.,' 45, p. 28, 1892.

† 'Recent Researches,' p. 126.

‡ 'Wied. Ann.,' 51, p. 225; also 52, p. 23.

That the cathode rays do carry a negative charge has been conclusively shown by Perrin,* and the method here used for testing the charge carried by the Lenard rays is the same in principle as that used by him on the cathode rays.

A tube as shown in fig. 1 was constructed.

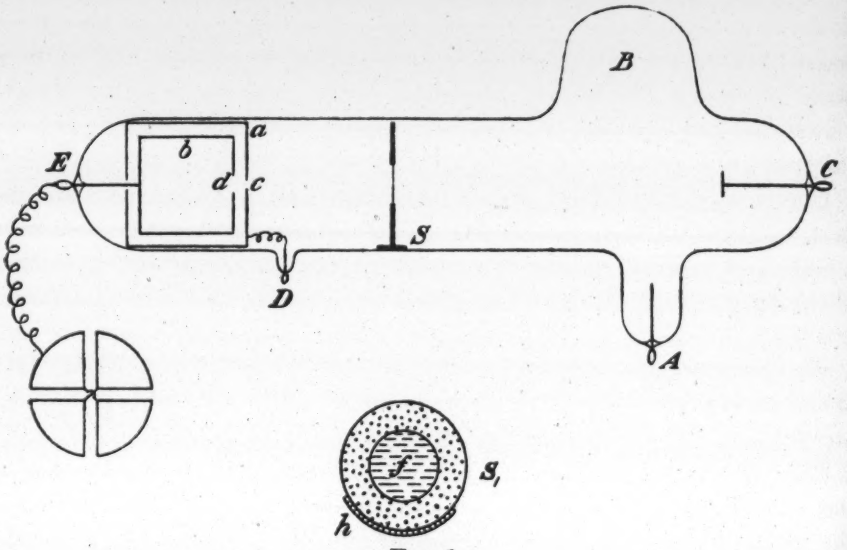


FIG. 1.

A is the anode and is connected to earth, C is the cathode.

Inside the tube are fixed two short metal cylinders *a* and *b*. The inner cylinder *b* has a small circular opening at *d*, facing towards the cathode, and is held in position by a stiff wire which passes through the glass at E, and is connected to a pair of quadrants of an electrometer.

If then the cathode rays entering the cylinder *b* carry a charge the electrometer detects it, even though the rays may not give up their charge to the metal walls of the cylinder.

The outer cylinder *a*, which has also a small opening at *c* facing the cathode, is kept connected to earth by means of a wire passing through the glass at D, and thus protects the inner cylinder from outside electrical action.

S is a screen (shown in front view at S_1), made of a circular disk of lead with the central part cut away, and the hole covered with a thin piece, *f*, of aluminium about 0·01 mm. thick.

The screen is mounted on a piece of iron, *h*, to enable it to be moved about in the tube by means of a magnet. The screen is of such a diameter as to almost touch the glass all round, just allowing freedom of motion under the action of the magnet.

* 'Nature,' January 30, 1896.

The wide side tube B is to enable the screen S to be pulled aside entirely out of the path of the cathode rays whenever it is desired to do so. The screen can easily be moved about by the magnet without disturbing the tube which is kept sealed on to the exhausting pump.

When the tube has been exhausted until the cathode rays are well developed, the screen is pulled into the side tube B, the cylinder *b* is connected to the electrometer, and the coil turned on. The electrometer shows a negative deflection, which rises rapidly to a point at which it remains constant. The passage of the discharge inside the tube breaks down to some extent the insulation between the cylinders *a* and *b*, and as the negative potential of *b* increases the rate of flow of negatively charged particles into it will diminish, until a state is reached at which *b* is losing charge to *a* at the same rate at which it is receiving charge by the flow into it of the charged particles, and at this point the potential of *b* remains constant.

In one experiment the deflection rose to 175 scale divisions, which showed that a capacity of about 300 C.G.S. units had been raised to a potential of 87 volts.

Everything else remaining the same, an electromagnet is placed with its poles on opposite sides of the tube between the cathode C and the cylinder *a*; when the magnet is on and the cathode rays deflected off the opening *c*, the cylinder *b* receives scarcely any charge, showing that the negative deflection previously observed is really due to the charge carried by the negative rays.

The screen S is now pulled from the side tube B, and placed in the path of the negative rays.

When the coil is turned on, a negative charge, smaller than previously, is still obtained in the cylinder *b*, and this charge is again not observed when the magnet is placed so as to deflect the rays off the screen, or so as to deflect the secondary rays from the screen off the opening *c*.

The screen was moved along the tube and the deflection measured in different positions with the following result.

The length of the tube from the cathode C to the cylinder *a* was 12 cms.

Distance of the aluminium screen from *a*.	Negative deflection.
1½ cm.	30
2 „	62
4 „	142
6 „	150
8 „	147
With screen in side tube B	175

These numbers show that with the screen 6 cm. from *a*, the

cylinder was raised to a potential 6/7 of that to which it was raised when the screen was away, and the cathode rays fell directly on the opening c. This must not, however, be taken as meaning that the flow of charged particles into the cylinder was almost as great with the screen as without it, because the potential to which b can be raised depends, as we have seen, on the conductivity of the air between a and b, and this with the screen in position is probably much less than without it, so that the flow of charged particles in the first case is much less than the numbers would indicate.

But the experiment does show that when the cathode rays fall upon a thin sheet of aluminium, we have a flow of particles away from the further side of the aluminium, which is deflected by a magnet, and which carries a negative charge.

The Lenard rays, therefore, like the cathode rays, carry a negative charge.

The negative potential to which the cylinder b is raised, depends on the position of the screen in the tube, as shown in the table given above. The intensity of the Lenard rays at a will depend on the distance of the screen from a, and also on the intensity of the cathode rays falling upon the screen.

When we consider the great amount of electricity carried by the negative rays (as we have seen, a few breaks of the coil charged 300 C.G.S. units to 87 volts, and other experiments of a different character on this point will be described later on) we can easily understand this stream of negatively charged particles away from the further side of the thin screen. This large negative charge suddenly brought up to the screen and there stopped, must cause a similar rush of negative charge away from the screen, especially as the air in the tube is in a condition suitable for a discharge to pass in it.

The screen in this experiment was not kept connected to earth, although in other similar experiments described below it was so connected. Nor does this appear to be of much consequence, because even if the screen is well earthed, the sudden rushes of negative charge up to it will cause variations in its potential so that it will not shield the portion of the tube on its further side.

In the experiments described above, the negatively charged secondary rays are collected inside the tube; it is more convincing to arrange the apparatus so that they can be collected outside the tube, as in this case it is easier to screen the collecting cylinder from outside electric forces.

For this purpose a tube, as shown in fig. 2, was constructed.

A circular brass cup, B, is made to fit closely on the end of the glass tube in which are fixed the terminals A and C. The brass cup has a small aperture e, 1 mm. in radius, in the centre

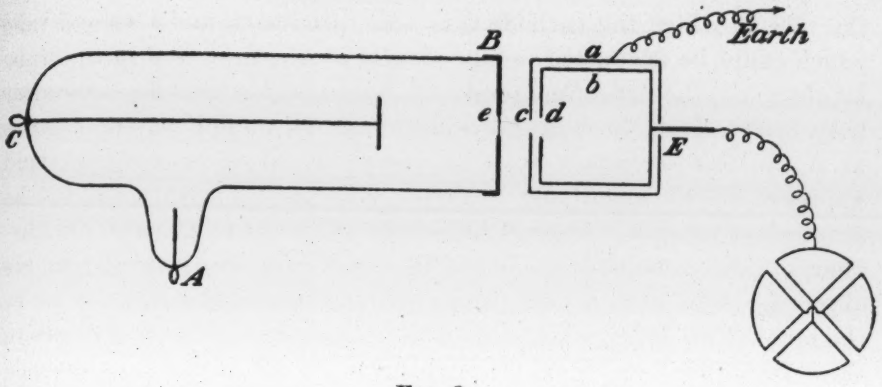


FIG. 2.

of its base. This hole is covered with a piece of oiled silk fixed on with sealing-wax, and the brass cup is then fastened on also with sealing-wax to the tube. Oiled silk was used for the window because with it no trouble is experienced in getting a piece thin enough, and at the same time free from holes. It easily bears an atmosphere pressure on an aperture 1 mm. in radius.

The brass cup is connected to earth and used as the anode; C is the cathode. A is an additional terminal which may be used as the anode. The cylinders *a* and *b* are arranged as in the last experiment.

When the openings are directed towards *e*, and not more than a few centimetres from it, the electrometer shows a negative deflection which slowly rises to a value at which it remains constant.

The cylinder *b*, with electrometer and connexions, had a capacity of about 250 C.G.S. units, and in one experiment was raised to a potential of 11 volts. The cylinder is not charged if, by means of a magnet, the cathode rays are thrown off the window *e*; neither is it charged if the cylinders are tilted a little so as not to point directly to *e*. The experiment was tried with a similar brass end of equal thickness (1·5 mm.), but with no window in it, and no charge was obtained.

The negatively charged rays given off from the window are soon stopped by the air at atmospheric pressure, and no effect is obtained when the cylinder is more than a few centimetres from the window. The experiment confirms that described above. The negative discharge from the window may be shown in another way.

A plate of ebonite is placed opposite the window at a centimetre or two from it, and the coil turned on for a few seconds. The plate is then dusted evenly over with a mixture of sulphur and minium, and tapped lightly to clear it of the powder which does not adhere to it. The well-known negative Lichtenberg figure is produced on the plate with its centre at the point opposite the window.

In Lenard's experiments he placed a screen of aluminium between

the tube in which the cathode rays were produced, and a second tube which could be exhausted independently of the first, and in this tube he observed the deflection produced by a magnet on the rays after they had passed through the aluminium, or, on our way of looking at it, on the secondary rays produced in this tube by the rapid approach of the negatively charged particles, in rushes, up to the aluminium window. Lenard found that if the conditions in the discharge tube remained the same, he could vary the pressure in his observing tube within wide limits without producing any change in the amount of deflection of the rays. This at first sight appears to show that the Lenard rays are different in character from cathode rays, because we know that when the pressure in a discharge tube is diminished the cathode rays are less deflected by magnetic force.

But when we diminish the pressure in a discharge tube we at the same time increase the difference of potential required to make the discharge pass, and if we regard the negative rays as negatively charged particles shot off from the negative electrode, this increase of potential difference will increase the momentum given to these particles, and consequently they will be less deflected. We may thus regard the change of pressure as changing the deflection not directly, but because it changes the difference of potential between the electrodes.

We cannot test this view, experimentally, inasmuch as we cannot vary the pressure and keep the difference of potential constant, but we can keep the pressure constant and vary the difference of potential to some extent. If a spark gap is placed in series with the discharge tube, by increasing the length of the spark we may make the difference of potential between the terminals of the tube greater than that required to make the discharge pass, and it is found that forcing up the potential difference in this way does diminish the deflection of the cathode rays.

In an experiment a cylindrical tube of 1·5 cm. radius was used with the cathode at one end. A central beam of cathode rays was deflected by a magnet on to the side of the tube so as to form a well defined patch of phosphorescence. When the spark gap was closed, this patch was at a distance of 38 mm. from the cathode. Increasing the length of the spark gap up to 10 mm. was sufficient to make the patch travel away to 44 mm. from the cathode.

This shows that without any change in pressure, an increase in potential difference is sufficient to diminish the deflection.

Now in Lenard's arrangement a change of pressure in the observing tube has no effect on the forces producing the rays in that tube, if we regard them as produced by the rapid pulses of charged particles up to the aluminium screen. The impulse starting the rays is not varied, and consequently we would expect no change in

the curvature of the rays. Of course in the denser medium we would expect the rays to lose their velocity sooner by impact with the gas particles, and Lenard himself found that in air at atmospheric pressure he could only observe the rays at a few centimetres from the window, while at lower pressures he could observe them at much greater distances.

In one of his experiments Lenard reduced the pressure so low in his second or observing tube, that he could not produce rays by connecting the coil directly to terminals in this tube. The pressure was so low that the discharge refused to pass. When the coil was connected to terminals in the primary tube the rays, even at this low pressure, were propagated in the second tube away from the aluminium window.

To explain this it is necessary to assume that the electromotive intensity produced by the impact of the charged cathode rays on the window is sufficient to produce discharge in the second tube, even when the coil connected to terminals in this tube cannot produce discharge. From the first experiment described above we calculate that at a single break of the primary of the coil the atoms striking on a square millimetre of area of a screen placed in the path of the cathode rays at a distance of 12 cm. from the cathode, carried a charge of about 60 C.G.S. units. A charge of this magnitude, brought suddenly up to the screen will produce very great electrostatic effects, and the electromotive intensity produced in the second tube may be great enough to dissociate the molecules close to the window, and produce discharge.

On the whole, the theory that the cathode rays are a stream of charged atoms explains satisfactorily the observed properties of these rays, and also the production and properties of the Lenard rays.

Seeing that the cathode rays produce some of the most interesting of the phenomena presented by a discharge tube, it is important to get some idea of the amount of the current actually carried by the rays. It is generally stated that the cathode rays play a comparatively unimportant part in the actual passage of the discharge, but although this may, to some extent, be true in the earlier stages of exhaustion, it is certainly not so at lower pressures.

By making use of the fact observed by several experimenters that the discharge does not always take the shortest path between the electrodes, we can separate out a part of the negative rays from the remainder of the discharge, and measure this part directly with a galvanometer, and thus get an inferior limit to the fraction of the whole current carried by the cathode rays. The appearance of the tube justifies us in assuming that the part of the current we thus separate out and measure is carried entirely by cathode rays.

A tube of the form fig. 3 was constructed, with the terminals A and C about 40 mm. apart. This tube has a narrow side tube B, as in figure, the shortest distance between the terminals by this path being about 160 mm.

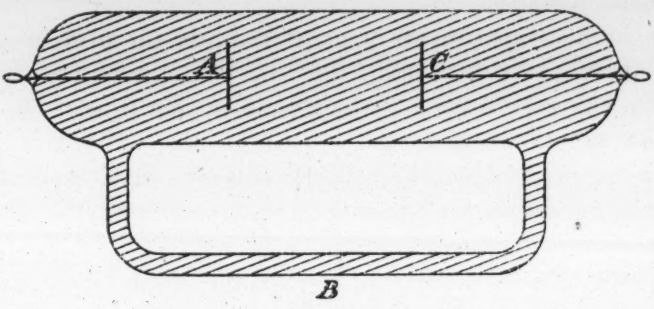


FIG. 3.

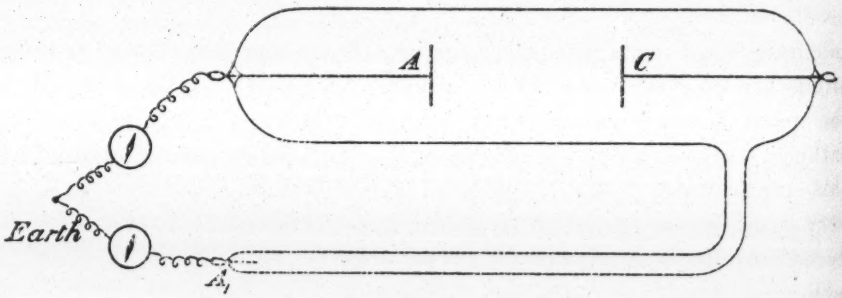


FIG. 4.

As the tube is being exhausted the discharge at first passes by the shortest path from A to C, but when the dark space round the cathode C spreads out until it reaches the walls of the tube, a point is reached at which the positive column starts behind the disk A, and travels by the side tube B.

The part between A and C which was formerly occupied by the positive column is now entirely free from glow, but there is still a stream of negative rays from C towards A. Of course, there is also a less intense stream of rays behind C, but still, if we measure the current carried by the rays from C to A, we have an inferior limit.

The reason why the much longer path by the side tube B is the path of least resistance, seems to be, because by that path the positive column is enabled to approach closer to the negative electrode, the intensity of the cathode rays being less behind C than from C towards A. When the positive column is travelling by the side tube B, if we bring a magnet up to the tube between A and C, so as to deflect the cathode rays on to the side of the tube near C, the whole

discharge again takes the shortest path AC, so that a field of magnetic force across the direction of discharge in this case diminishes the resistance of the tube, by diminishing the length of it which is occupied by the negative dark space and allowing the positive column to lengthen out.

A slight variation in the tube (fig. 3) enables us to measure the current carried by the cathode rays in CA. The tube is made as in fig. 4. C is the cathode, A and A_1 are both connected to earth, and the anode of the coil is also to earth.

At first the discharge passes between A and C, but when the proper exhaustion is reached the positive column starts from A_1, and travels round by the longer path.

We can insert a galvanometer between A or A_1 and the earth connection, and measure the current through AC and A_1C.

The galvanometer used was a very sensitive one shunted with a small resistance negligible in comparison with that of the discharge tube.

The results obtained were as follows:—

Before the positive column changed from the large tube to the side-tube B, no current could be detected in the latter. A very small diminution of pressure causes the positive column to travel by A_1C, and now the current in AC was about 1/50 of that in A_1C.

The pressure was now gradually diminished and readings taken at every step until very little glow was visible in A_1C, while the phosphorescence in AC was steadily increasing.

The ratio of the current in AC to that in A_1C meanwhile steadily increased, until finally it was 1/4.

These numbers show that even at a pressure at which there is little phosphorescence a considerable fraction (more than 1/50) of the whole discharge was carried away from the cathode by the negative rays, while at lower pressures these rays carried a large portion of the discharge.

The fact that the negative rays travel in straight lines away from the cathode, independently of the position of the anode, and therefore, in a discharge tube in which the anode is not in the line of the rays, do not directly convey their charge to the anode, does not necessarily prove that the part of the discharge which they carry away from the cathode is small.

In such a tube there will be an excess of negative electricity at the point where the cathode rays are stopped by the glass, and this makes its way to the anode by a current in the gas, which is a good conductor when the primary discharge is passing.

In conclusion, I wish to thank Professor Thomson for many valuable suggestions in the course of the above work.

"On the Application of Harmonic Analysis to the Dynamical Theory of the Tides. Part I. On Laplace's 'Oscillations of the First Species,' and on the Dynamics of Ocean Currents." By S. S. HOUGH, M.A., Fellow of St. John's College and Isaac Newton Student in the University of Cambridge. Communicated by Professor G. H. DARWIN, F.R.S. Received March 12,—Read April 8, 1897.

(Abstract.)

1. By a transformation of the differential equations of Poincaré* for the oscillations of a rotating mass of liquid, an equation for the tidal oscillations of the ocean is obtained in a form similar to that employed by Laplace.† From this equation it is deduced that if the surface-value of the disturbing potential which gives rise to the oscillations is expressible as a series of zonal harmonics in the form

$$\Sigma\gamma_n P_n(\mu),$$

then, provided the depth be a function of the latitude alone, the height of the surface-waves will also be expressible as a series of zonal harmonics; if this series take the form

$$\Sigma C_n P_n(\mu),$$

the following relation connecting successive C's is shewn to hold good when the depth is uniform:—

$$\frac{C_{n-2}}{(2n-1)(2n-3)} - C_n\left\{\frac{\lambda^2/4\omega^2-1}{n(n+1)} + \frac{2}{(2n-1)(2n+3)} - \frac{hg}{4\omega^2a^2}\left(1-\frac{3\rho}{(2n+1)\sigma}\right)\right\} + \frac{C_{n+2}}{(2n+3)(2n+5)} = \frac{h\gamma_n}{4\omega^2a^2} \quad \ldots\ (\alpha),$$

where λ denotes the "speed" of the oscillation dealt with, ω the angular velocity of rotation, ρ, σ the density of the water and the mean density of the earth respectively, h the depth, and a the earth's radius.

A similar relation connecting three successive C's is also shewn to hold good if the depth be a function of the latitude given by the formula

$$k+l(1-\mu^2),$$

where $\sin^{-1}\mu$ denotes the latitude.

2. If we put all the γ's zero in the formula (α), we may eliminate

* 'Acta Math.,' vol. 7, p. 356.

† 'Méc. Cél.,' Part I, book 4, cap. i.

the C's and obtain an equation for the determination of the periods of free oscillation of zonal type. This equation will in general be of a transcendental character, and will be expressible by means of non-terminating continued fractions. The convergence of the continued fractions which occur is, however, very rapid so long as the depth of the water is not materially less than that which occurs on the earth, and this fact enables us to approximate to the roots numerically by a process of trial and error with great facility. The type of oscillation as indicated by the height of the surface-waves is then determined by calculating the ratios of successive C's by means of the formula (α). Numerical results are given in the paper for four different values of h, corresponding to depths of about 7260, 14,520, 29,040, and 58,080 feet. For these depths, the longest periods of free oscillation of symmetrical type (corresponding to even values of the suffixes n) are found to be—

18 hrs. 3 mins., 15 hrs. 11 mins., 12 hrs. 29 mins., 9 hrs. 52 mins.,

while for the unsymmetrical types the longest periods are—

30 hrs. 29 mins., 25 hrs. 28 mins., 21 hrs. 0 mins., 16 hrs. 52 mins.

3. If we retain the γ's in the equations (α), these equations will serve to determine the C's in terms of the γ's, and thus to evaluate the height of the forced tides resulting from a given disturbing force. The most important application is that in which all the γ's are zero except γ_2, and where λ is small in comparison with ω. The method of procedure is then similar to that adopted by Professor Darwin* for the discussion of the long-period tides, and the numerical results are found on comparison to agree with his, but the analytical form in which they appear is different. The series of zonal harmonics by which the tide-heights are expressed always converge with greater rapidity than the power-series of Professor Darwin, while we have the additional advantage of being able to include in the analysis the effect of the gravitational attraction of the water. Some further numerical results relatively to the forced tides in an ocean of variable depth are also given in the paper.

4. The series obtained for the forced tides of long period indicate that the tides do not tend towards their "equilibrium" values when the period of the disturbing force is prolonged. This circumstance, which at first sight appears to be at variance with the general laws of oscillating systems, has been explained by Professor Lamb† as a consequence of the fact that the system is capable of free oscillations of infinitely long period, or free steady motions. We probably have examples of these free steady motions in the large

* 'Encyc. Brit.,' art. Tides, § 18.

† 'Hydrodynamics,' § 198.

ocean currents, and it is therefore of interest to examine the nature of the free steady motions which could exist on a rotating globe. It is shown that so long as the depth is uniform the only steady motions possible are those in which the particles of water move along parallels of latitude, while, if the depth be variable, the stream lines of the possible steady motions coincide with the lines

$$h/\mu = \text{const.},$$

where h denotes the depth and μ the sine of the latitude.

5. These currents differ materially from the actual currents of the ocean, and the difference is probably to be explained from the fact that the density of the water has been treated as uniform, whereas the density of the ocean varies from point to point in consequence of differences of temperature, salinity, &c. These variations of density have been claimed by some authorities as the sole cause by which ocean currents are maintained, but as free currents could exist, and would almost certainly be excited by such causes as the winds, even if there were no variations in the density, it is improbable that such is the case, though there can be no doubt that the density-variations play an important rôle in modifying the possible forms of the free steady motions, and thus determining the course which the currents pursue.

6. Though these currents have been described as "free," they could not exist in nature without some disturbing force in consequence of the action of viscosity. If, however, the coefficient of viscosity of the water be sufficiently small, a very small amount of disturbing force will be sufficient to excite and maintain them. As an example of the manner in which they could be maintained by a small disturbance, an attempt is made to estimate the effects of the unequal distribution of rainfall and evaporation over the surface of the earth. It is shewn that this cause will give rise to a slow current from the poles towards the equator of quite insensible amount, and not changing from time to time; but, in addition to this flow, there will be set up currents along the free stream lines which, in the absence of friction, would increase uniformly with the time. When the water is viscous, the latter currents will, however, eventually attain a magnitude equal to that which they would acquire in a period equal to the modulus of decay of the type of motion set up, after which they will remain steady. Such currents might attain a magnitude comparable with the actual currents of the ocean if the modulus of decay of any of the types of current motion be as long as twenty years, and as this value for the modulus of decay is not improbable, it is concluded that such causes as the unequal distribution of rainfall are sufficient to give rise to currents quite comparable with those which exist on the earth.

"On Boomerangs." By G. T. WALKER, M.A., B.Sc., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received March 15,—Read April 8, 1897.

(Abstract.)

A typical returning boomerang resembles in general outline a symmetrical arc of a hyperbola, and is about 80 cm. in length measured along the curve. At the centre, where the dimensions of the cross section are greatest, the width is about 7 cm., and the thickness 1 cm.

Of the two faces, one is distinctly more rounded than the other; in addition the arms are twisted through about 4°, in the same manner as the blades of a right-handed screw propeller.

Such an implement, if thrown with its plane vertical, will describe a circular path of 40 or 50 metres in diameter, rising to a height of from 7 to 12 metres, and falling to the ground with its plane of rotation horizontal at a point somewhere near the thrower's feet.

The flight may be regarded as a case of steady motion, of which the circumstances gradually vary. In the more complicated, as well as the simpler, paths, observation makes it clear that everything depends on the changes in direction and inclination of the plane of the boomerang, and that the character of these changes is always the same; if they can be explained theoretically, the peculiarities of the motion may be accounted for.

Since the effects of the different forces at work are conflicting, it is necessary to adopt quantitative methods, even if the degree of accuracy attainable is not high; accordingly ratios comparable with a tenth are treated as small, and their squares neglected.

If we regard the boomerang as a thin, slightly distorted lamin, and integrate over it the forces indicated in S. P. Langley's paper on "Experiments in Aerodynamics,"* we can obtain equations of motion. From these, treating the motion as steady (to the first approximation), we may deduce the values of the angular velocities on which the direction of the axis of rotation depends. Five cases are worked out numerically, and the various effects of the "rounding" and "twisting" agree in character with the experimental facts; the discrepancies in actual magnitude are not larger than might, from the nature of the case, have been anticipated.

The theoretical results may be further tested by applying them to determine the conditions favourable to the production of other flights in which, after the first circle, a loop is described, either in

* 'Smithsonian Contributions to Knowledge,' 1891.

front of or behind the thrower; in each of these cases success has been attained. An explanation is also afforded of the returning of a boomerang without "twist," made by Mr. O. Eckenstein, and of the wonderfully long, straight trajectories of some of the native non-returning implements.

"Condensation of Water Vapour in the presence of Dust-free Air and other Gases." By C. T. R. WILSON, B.Sc. (Vict.), M.A. (Cantab.), of Sidney Sussex College, Clerk-Maxwell Student in the University of Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received March 15, Read April 8, 1897.

(Abstract.)

In a note read before the Cambridge Philosophical Society (May 13, 1895) I stated, as the result of some preliminary experiments, that when air, originally saturated with aqueous vapour, undergoes sudden expansion exceeding a certain critical amount, condensation takes place in the form of drops throughout the moist air, even in the absence of all foreign nuclei.

The present paper contains an account of the measurements which were afterwards made of this critical expansion in air and other gases, as well as of further phenomena which have since been observed in connection with the condensation of aqueous vapour from the supersaturated state.

Two different forms of expansion apparatus have been used. Both were designed to enable a given sample of the saturated gas to be suddenly expanded as often as might be desired without any risk of foreign nuclei entering. All such nuclei originally present were removed by repeatedly forming a cloud by expansion and allowing it to settle, till expansions of moderate amount ceased to cause any visible condensation. In both forms of apparatus a definite expansion of any desired amount could be produced. They were designed to give an exceedingly rapid expansion, the rate at which the volume was increasing being greatest, too, just before the expansion was completed, when the temperature was lowest and the influx of heat from the walls most rapid; so that, as indeed appears from the constancy of the results obtained, the theoretical lowering of temperature must have been very nearly reached.

The two machines, in spite of the fact that the volume of the air in the first was twenty times as great as that contained in the second, gave identical results. The larger machine was only used in the experiments on air.

If air, initially saturated and free from all foreign nuclei, be suddenly allowed to expand, a rain-like condensation results if the ratio of the final to the initial volume, v_2/v_1, exceeds 1·252; no condensation taking place except on the walls of the vessel with smaller expansions.

When v_2/v_1 exceeds 1·252 the condensation continues to be rain-like, showing over quite a wide range no visible increase in the number of the drops with increasing expansion, till a second quite definite limit is reached when v_2/v_1 lies between 1·37 and 1·38. With expansions greater than this the condensation is cloud-like, the number of the drops which are formed increasing at an enormously rapid rate with increasing expansion. If v_2/v_1 be made successively greater and greater, a very definite series of colour phenomena is observed as this ratio is increased from 1·38 to 1·44, indicating a very rapid diminution in the size, and corresponding increase in the number of the cloud particles with increasing supersaturation. Only the smaller apparatus was used in the experiments on the cloud-like condensation, the time taken for the expansion to be completed being much shorter than in the other.

Experiments were made upon the condensation phenomena in the presence of oxygen, hydrogen, nitrogen, chlorine and carbonic acid.

Of these, all except hydrogen were alike in showing the two forms of condensation, each requiring a definite degree of supersaturation to produce it.

Chlorine is too far removed from the condition of a perfect gas to enable the lowering of temperature and resulting supersaturation to be calculated. This can be done, however, in the case of the other gases.

By the supersaturation is here meant the ratio of the actual density of the vapour when the expansion has just been completed, and the minimum temperature has therefore been reached, to the density of the vapour in equilibrium over a flat surface of water at that temperature.

We may summarise the results obtained with the various gases, including air, as follows:—

In order that rain-like condensation may result in the presence of any of these gases, except hydrogen, the expansion must be sufficient to cause the supersaturation to exceed a certain value, amounting, when the final temperature is −6° C., to between 4·2 and 4·4, and diminishing with rising temperature.

In order that cloud-like condensation may take place in the presence of any of these gases, including hydrogen, the expansion must be sufficient to cause the supersaturation to exceed a certain value, amounting, when the final temperature is about −16° C., to 7·9.

When the supersaturation reached lies between these limits rain-

like condensation results in all the gases, except hydrogen, in which scarcely any trace of condensation is seen when the supersaturation is even slightly below 7·9.

A statement of the effect of the Röntgen rays upon condensation in the presence of air was given in a note read before the Royal Society, on March 3, 1896. The rays have the effect of greatly increasing the number of the drops, the minimum expansion required to cause condensation being unaltered.

Experiments have been made upon the effect of these rays when hydrogen is substituted for air. Their effect on the moist hydrogen is to introduce nuclei, which only require the supersaturation to reach the same limit as is required for rain-like condensation in air and the other gases, in order that condensation may take place upon them. In all these experiments the X-rays had to pass through glass to reach the gas, and must have been thereby very much reduced in intensity, yet their effect in the case of hydrogen was quite noticeable when the bulb producing them was 120 cm. from the expansion apparatus.

The nuclei which bring about the rain-like condensation are equivalent, in their power of helping condensation, to water drops of $8{\cdot}6 \times 10^{-8}$ cm. in radius, that is, water drops of this size would just be able to grow in vapour supersaturated to the extent actually required to bring about condensation. This number is calculated on the assumption that Boyle's law holds for the supersaturated vapour, and that the surface tension retains its ordinary value even in such small drops. It must, therefore, only be considered as a rough approximation.

On the same assumptions, the nuclei which are able to act as centres of condensation when the supersaturation is sufficient to cause the cloud-like condensation, are equivalent to drops of $6{\cdot}4 \times 10^{-8}$ cm., or less, in radius. They are present, as we have seen, in all the gases tried, when saturated with aqueous vapour, and are exceedingly numerous. It is probable, therefore, that they are actually small water particles, such as one would expect to come into existence momentarily through encounters of the molecules.

The nuclei which bring about the rain-like condensation are always few in number, and they appear to be entirely absent in hydrogen; their number, therefore, depends on the nature of the gas. They are probably, therefore, of a different nature from those which come into play when the supersaturation is great enough to cause the cloud-like condensation.

"Double (Antidrome) Conduction in the Central Nervous System."

By C. S. SHERRINGTON, M.A., M.D., F.R.S., Holt Professor of Physiology, University College, Liverpool.

Received February 15,—Read April 8, 1897.

In a paper presented to the Society last year,* I drew attention to some striking instances of "long conduction" through the bulbospinal cord, and among others to the following singular one. If after transection over the bulbospinal axis the *funiculus gracilis* be excited, *e.g.*, at the *calamus scriptorius*, the excitation evokes movement (contraction, relaxation) in the idiolateral hind limb. If instead of *f. gracilis* the *funiculus cuneatus* be excited the movement (contraction, relaxation) is in the idiolateral forelimb. The movement in the hind limb is in the monkey usually adduction and flexion of hallux, in the cat flexion of knee, hip, or ankle. In the monkey the forelimb movement is usually flexion and adduction of pollex, often with extension of the other digits; in the cat, more usually flexion of elbow with protraction of the shoulder. The movements which occur are, however, various, and I will here only add that those from the *f. gracilis* include the vaginal and anal orifices, the tail, and the abdominal muscles, those from *f. cuneatus* the diaphragm; but that neither from *f. gracilis* nor *f. cuneatus* have I obtained idiolateral extension of elbow or of knee.

In my former paper this phenomenon was recorded under the head of "long bulbo-spinal conductions." I did not offer any explanation of it, for it appeared to particularly require further investigation. I have, since communicating the above paper, taken various opportunities of examining the reaction further. My enquiry has elicited the subjoined results.

The movements evoked in the perineum or hind limb by excitation of the *f. gracilis* after transection of the bulb are obtainable from that column, after its isolation, by freeing it above and from its ventrolateral connexions for a length of 3 cm., and then suspending its upper end from a thread. The reaction is therefore hardly due to escape of the stimulating currents used, so that they reach the lateral columns and the descending tracts there contained. The currents employed have been induced, and of an intensity imperceptible, or barely perceptible to the tongue tip. The electrodes have been bright steel needles, placed about 1 mm. apart, and laid on the surfaces of the cord or bulb.

* "Experiments in examination of the Peripheral Distribution of the Fibres of the Posterior Roots of some Spinal Nerves, Part II," Abstract, 'Roy. Soc. Proc.,' vol. 60, No. 365.

The reaction is obtainable when the transection has been made altogether below the *nuclei graciles et cuneati*. It therefore does not necessarily involve the cells of those nuclei.

The reaction is not prevented by complete bilateral transverse severance of the ventro-lateral columns and grey matter of the cord at the—

5th cervical root level,
nor at the 8th „ „
„ 5th thoracic „
„ 1st lumbar „
„ 5th „ „ (cat).

The reaction is at once annulled on severance of the dorsal columns at any one of the above levels, although at the same time the ventro-lateral columns and the grey matter remain intact.

The reaction from the left *f. gracilis* is annulled by severance of the left dorsal column, that of the right by the severance of the right.

The reaction can almost always be obtained—although incompletely—by mechanical excitation, *e.g.*, by compression with ivory forceps, often even by a mere touching with the forceps.

That the conduction involved in the reaction does not implicate the fibres of the pyramidal tract—which at first instance suggest themselves as a source of fallacy—seems clear in light of the above. That supposition is also, and I think finally, excluded by the following observation. I have found the "long intra-spinal reflexes," like sub-cerebral rigidity ("decerebrate tonus,"*) locally abolished, or greatly depressed by total severance of the sensory spinal roots belonging to their own region of terminal discharge. Thus, to take an instance given in my former paper, if the right forepaw be stimulated the paths of "short spinal conduction"* from it lead to discharge of its own flexors of elbow, extensors of wrist, &c., as specified in the paper; and the paths of "long spinal conduction" from it lead to discharge of the muscles of the *idiolateral* hind limb. To evoke from the forepaw movement of the contralateral hind limb is relatively difficult; this contralateral movement is less commonly and less easily obtained, and when obtained less vigorous, less prolonged, and usually commences later than the idiolateral. But if, in the instance taken, the series of afferent spinal roots belonging to the right hind limb be severed, and stimulation of the forepaw (right) be then repeated, the movement induced in the hind limbs is contralateral, *i.e.*, a *crossed* one. In the

* "Experiments in examination of the Peripheral Distribution of the Fibres of the Posterior Roots of some Spinal Nerves, Part II," Abstract, 'Roy. Soc. Proc.,' vol. 60, No. 365.

idiolateral limb it is extremely difficult, often impossible, to then obtain by this facile long spinal path any discharge at all on the side of the transected afferent roots, although that side is usually peculiarly accessible. I find that similarly severance of the dorsal (afferent) roots in their extraspinal course greatly impairs the reaction from the *f. graciles* and *f. cuneati*. Thus, if when flexion of right knee or right hallux is being regularly evoked by excitation of *f. gracilis* at the top of the cord, the extraspinal dorsal (afferent) roots of the right pelvic limb be severed, the reaction, until then regularly obtained, disappears or almost disappears. The section of right hand roots annuls the right hand reaction, but not the left hand, and conversely. On the other hand, the flexion of knee, or of hallux, or of elbow obtained by excitation of the Rolandic cortex or of the lateral column (pyramidal tract fibres) is, as has been shown in a previous number of these 'Proceedings'* by Dr. Mott and myself, not impaired after the root severance, indeed often appears, on the contrary, to be facilitated. In this respect, therefore, the reaction obtainable by direct excitation of *f. graciles* and *f. cuneati* is shown to be curiously different from that obtainable from the pyramidal tract fibres and Rolandic cortex. On the other hand, it is seen to resemble in this respect to a remarkable degree the "long spinal reflexes" as defined above.

What, then, is the nature of this reaction obtainable from the *f. graciles* and *cuneati?* The reaction is evidently one which involves each dorsal column of the cord as a conducting path, in many cases as a "long"—in not a few as a remarkably long—conducting path, even employing its whole length. In light of the evidence given above, I infer that although certainly, as has been long established, the dorsal column is, with the single exception of its short, scanty, and deeply placed ground-bundle, a functionally purely *upward* path, consisting of nothing else than sensory root fibres, the vast majority of which fibres—and all the longest of which—are ascendant; the conduction along it in these experiments is *downward*, even extending its whole length. That is to say, the conduction must be downward and cellulipetal along ascending axons which function in a cellulifugal direction; that is to say, the propagation of the impulses artificially started in my observations must have been *antidrome* instead of *orthodrome*. The motor discharges evoked I refer to the spread of the excited condition into the collaterals of the axons excited to antidrome conduction, their collaterals impinging upon motor neurons.

The direction of propagation occurs therefore in opposition to the

* "Experiments upon the Influence of Sensory Nerves upon Movement and Nutrition of the Limbs," 'Roy. Soc. Proc.,' vol. 57, March 7, 1895.

law of the "*polarisation dynamique des nevrons*" put forward by Ramon-y-Cajal* and V. Gehuchten.† It offers, however, no contradiction to what James‡ has termed "the law of forward direction;" it only emphasises that that law predicates the existence of at least two links in its conduction-gear.

The reaction is therefore in my view an extreme illustration of double (antidrome, *doppelsinnige*) nervous conduction. After du Bois' fundamental observation with frog's sciatic and the electrical sign, it has been Kühne's *sartorius* experiment,§ and Babuchin's‖ reversed discharge in the electric organ nerve-fibre, which have laid a satisfactory foundation for double conduction in peripheral nerves. But between those experiments and these, the subject of this note, there are, it is true, differences. In the latter, (α) propagation occurs over relatively huge distances and (β) the reaction occurs within the field of the central nervous system. These differences need not, however, negative the relationship of the phenomena. They render it the more instructive.

It is obvious that there must be opportunity for detection of antidrome conduction in parts of the central nervous system besides the dorsal spinal columns. Thus, on exciting, especially with electric currents, the mammalian metencephalon (*vermis cerebelli*) and *isthmus rhombencephali*,¶ subsequent to ablation of the parts above, I have seen movements produced in the limbs and trunk, and also inhibitions occur. Thus, in instance of the latter, inhibition of the tonic extensor spasm of the fore and hind limbs combined with contraction of the flexors of knee and elbow, such as is seen under local spinal reflex action.** It will have to be determined whether in such cases as the former we have not before us instances of antidrome conduction along ascending paths. The antidrome phenomenon, while of valuable assistance when recognised, may, if unrecognised, give rise to very misleading inferences. Its methodic use should place in our hands a fresh instrument of value for neurological research.

* 'Medicina practica,' 1889; 'Revista de Ciencias Medicas de Barcelona,' Nos. 21 and 22, 1891.

† 'La Cellule,' vol. 7, p. 101, 1891.

‡ 'Psychology,' and *cf.* Waller, 'Science Progress,' vol. 3, p. 186, May, 1895.

§ 'Arch. f. Anat. u. Physiol.,' 1859, p. 595.

‖ Du Bois-Reymond's 'Arch. f. Physiol.,' 1877, p. 66.

¶ Sherrington, 'Roy. Soc. Proc.,' vol. 60, p. 414.

** Sherrington, *ibid.*

"Further Note on the Sensory Nerves of Muscles." By C. S. SHERRINGTON, M.A., M.D., F.R.S., Holt Professor of Physiology in University College, Liverpool. Received February 26,—Read April 8, 1897.

In a former number* of these 'Proceedings' I drew attention to the occurrence of reflex reactions evoked by mechanical and electrical excitation of individual eye-muscles and of their nerve-trunks. I was later somewhat surprised when, after the sensory nature of the structures originally termed muscle-spindles (Kühne) had been proved,† I was unable to find in the eye-muscles any examples of these structures.‡ I had expected to find in those muscles, on account of the great delicacy of their control and co-ordination, and in view of the well-known richness of their innervation, a field peculiarly favourable for the examination and study of "spindles." It appeared to me possible, however, that spindles if of a *very* simple type, *e.g.*, containing a single muscle fibre not enveloped in a distinct capsule, but with simply an unthickened perimysial sheath and without any circumfusal lymph space, in short if reduced to a far simpler type than I have ever met actually existent, might, although present, yet escape recognition. I turned, therefore, to the production of degeneration for further information.

I had noted that the intrafusal muscle-fibres, of the "red" variety as they are, undergo when the nerve-trunk of a muscle has been severed, a much slower course of alteration than do extra-fusal muscle fibres,§ *i.e.*, I found no pronounced degeneration for even two years following section. I therefore cut through *n. oculomotorius* at its origin, and examined the resultant degenerations in the eye-muscles which it innervates and in their individual nerve-trunks. The degenerative process was allowed scope for various periods. This method of test failed, however, to give me distinct results because (1) the muscle-fibres of the eye-muscles (in monkey) exhibit normally a certain variable amount of fatty granulation, simulating degenerative change; and because (2) the resultant changes in the muscle-fibres, although the fatty granulation distinctly increased, did not even after a period of sixty days show the clearly distinctive characters I had hoped.

On the other hand, in the nerve-trunks, extra-muscular and intra-

* 'Roy. Soc. Proc.,' vol. 52. April, 1893. Further note on the "Correlation of the Action of Antagonistic Muscles."

† 'Proc. Physiol. Society,' No. 3, 1894.

‡ *Ibid.*, and 'Journal of Physiology,' vol. 17, 1894. "The Constitution of the Nerves of Muscles."

§ 'Journ. of Physiology,' vol. 17, 1894, *ibid.*

muscular, the Wallerian degeneration did clearly demonstrate a result of importance. With the exception of a few minute fibres, of variable number, derived perhaps from the ciliary ganglion, *all* myelinate nerve-fibres in all these eye-muscles were degenerated. Therefore these eye-muscles derive the vast majority of their myelinate nerve-fibres from *n. oculomotorius*. The sensory innervation of these muscles does not therefore seem derivable from the Vth cranial pair. In accord with this I found (α) that severance of both trigemini caused no obvious impairment of the movement of the eye-balls, (β) that the combined severance of both *nn. trigemini*, and of both optic nerves even after section of the encephalic bulb did not severely depress the tonus of the eye-muscles. Now we know that section of the sensory spinal roots belonging to muscles does very severely depress the tonus of them.

At the same time I was struck with the long distance to which many of the nerve-fibres in these muscles travel forward toward the ocular tendons of the muscles. I was the more impressed with this fact because direct examination proved that the region of the distribution of motor end-plates in these muscles is almost confined to the middle portion of the fleshy mass of the muscle. Further investigation of the course and destination of the nerve-fibres at the tendon end of the muscle revealed them (both in cat and monkey) undergoing terminal subdivision, and in very numerous instances passing beyond into the bundles of the tendon itself. The terminations of many of these nerve-fibres lie within the tendons; many recurve again toward the muscular fibres, and end just at junction of muscle-fibre with tendon bundle. The nerve-fibres in so terminating frequently become thick—as I have described in the case of muscle-spindles—with shortened internodes.

The terminal arborisation which the nerve-fibres finally make is as a rule small as compared with the end-arborisation of ordinary Kühne-Ruffini "spindles" or the Golgi "tendon-organs," but closely resembles in numerous instances the form of arborisation of the latter.

In my former communication I wrote*—"The question therefore arises whether the above cranial nerves (IIIrd and IVth) are not in reality sensori-motor." In view of the additional observations now recorded I think it must be conceded that *nervus oculomotorius* is perhaps not a merely motor nerve, but although purely "muscular" may be sensori-motor.

My observations have included also the IVth cranial pair, and with like result. Investigation of the VIth cranial pair is also in progress.

It also appears clear from the above that the absence of the dis-

* 'Proc. Physiol. Society,' No. 3, 1894.

tinct Kühne-Ruffini "spindles" from a muscle does not exclude the possession by it of sensorial end-organs, and of afferent nerve-fibres. This point is not without importance, because examination of various muscles has led me to the conclusion that the "spindle-organs" are absent from the following muscles:—From all the orbital eye-muscles, from the intrinsic muscles of the larynx (though Pacinian corpuscles occur in these as in various other muscles), from the intrinsic muscles of the tongue, and from the diaphragm. It is notable that all these muscles belong to that set which are innervated by nerve-fibres of rather smaller calibre (Gaskell) than those supplying the skeletal muscles generally, that is to say, are innervated by the non-ganglionated splanchnic efferent nerves of Gaskell.

"On the Breaking-up of Fat in the Alimentary Canal under Normal Circumstances and in the Absence of the Pancreas." By VAUGHAN HARLEY, M.D., M.R.C.P., Professor of Pathological Chemistry, University College, London. Communicated by Professor HORSLEY, F.R.S. Received March 18,—Read April 8, 1897.

(From the Department of Pathological Chemistry, University College, London.)

In a previous paper* I discussed the simple absorption of milk fat from the alimentary canal of dogs, and compared the results with a series of dogs in which the pancreas had been previously removed. Fasting dogs fed on milk absorbed no less than 21 to 46 per cent. of the total fat given in seven hours, while in those where the pancreas had been entirely removed some two days previously, there was, during that space of time, no evidence of any absorption whatever from the alimentary canal.

The fact that no marked absorption of fat occurred in dogs after the extirpation of the pancreas seems to confirm the old view that the pancreatic secretion was necessary for absorption.

This alleged action of pancreatic juice in preparing fat for its absorption from the alimentary canal, is usually supposed to be due to the secretion containing, firstly, a fat-splitting ferment, which, by breaking up part of the neutral fat into free fat acids and glycerine, leads to the first stage of emulsification; and, secondly, alkaline sodium carbonate, which combines with some of the freed fat acids to form a soap, thus rendering the emulsification of the fat a simple matter—as can easily be demonstrated in a test-tube—and at once facilitates its absorption through the intestinal walls.

* 'Journal of Physiology,' 1895, vol. 18, p. 1.

The manner in which altered or emulsified fat may traverse the intestinal or mucous membrane is not discussed in the present paper, but only the manner and degree of breaking up of ingested milk fat, and the part played in such a process by the pancreas. My former paper showed that there was a great decrease in the absorption of fat when the pancreas was removed, and it is my present purpose to show how much of this is due to the absence of either the fat-splitting ferment, or of the alkaline sodium carbonate of the pancreatic secretion.

It is not necessary here to again draw attention to the extracts from the literature of this complicated subject, which I quoted in the previous paper, and which showed that there was reason to believe that neutral fat not only could split up fat acids and glycerine in the absence of pancreatic secretion, but even form soaps. The present research has for its object the precise determination of this point.

The results obtained may therefore be divided into a normal and a pathological, *i.e.*, according as to whether the pancreatic secretion is present or absent. Thus :—

1. The normal splitting up of fat in the various parts of the alimentary canal in dogs on a milk diet.

2. The effect of removal of the pancreas on the splitting up of fat in dogs on a milk diet.

Before discussing the results, it will be as well to give briefly the method employed.

Method of Investigation.

1. *Operative Procedure.*—The animals were in all cases treated in practically the same manner. The "normal" dogs were allowed to fast for a period of four days, during which time the rectum was washed out with a copious warm water enema, glycerine being afterwards given so as to aid the expulsion of the water. In the cases where the influence of the pancreas on the breaking up of fat was to be investigated after two days' fast, the pancreas was entirely removed under ether, and then the animals allowed to fast another two days, during which time the bowels were again washed out.

A given quantity of milk was administered in the morning, and seven hours later the dogs were killed by means of chloroform. The abdomen was quickly opened, and ligatures applied to the œsophagus, the pylorus, just above the cæcum, and as low down as possible round the rectum. The contents of each part of the alimentary canal thus isolated were then carefully collected and separately analysed.

The milk given in each case was concentrated to a convenient quantity by boiling; a specimen of this was analysed, so that the

absolute composition of the fats present in the milk at the time of administration was accurately known, and could be compared with that in the various parts of the alimentary canal.

2. *The Chemical Analysis.*—The substances collected from each separate part were dried at 90° C., and then placed in a Soxhlet's apparatus and extracted with ether until the ether was unable to extract any more soluble matter. The ether extract was then dried and re-dissolved in absolute ether of sp. gr. 0·720, and filtered through a fat-free filter paper into a weighed vessel, in which it was dried and afterwards weighed.

The ether residue thus obtained was then treated with a watery solution of sodium carbonate, which converted the free fat acids into soaps, and after again drying, the neutral fat, cholesterin, &c., was again extracted with ether and weighed. The difference between this and the first weighing was taken as equal to the quantity of free fat acids present.

In some few cases quantitative estimation of the cholesterin was made by saponifying the neutral fat extract with alcoholic sodium hydrate, evaporating to dryness, and then extracting with ether. This being repeated until saponification with alcoholic sodium hydrate no longer removed any neutral fat, and it was found that the ether extract consisted of cholesterin alone.

The original substance which had been extracted with ether was treated with dilute hydrochloric acid so as to liberate any fat acids from the soaps that might be there present. It was then dried at 90° C., and when completely dry was again extracted with ether. The quantity of ether extract thus obtained represented the fat acids present in the form of soaps.

The total fat was, therefore, obtained by adding the quantity of fat acids obtained from the soaps to the former ether extract.

To facilitate the comparison of the results in the experiments they are given in tabular form, and the analysis of the diet in each part of the alimentary canal is divided into groups, *i.e.*, neutral fat, fat acids, and fat acids combined as soaps.

The neutral fat includes the cholesterin, which was not in the majority of cases separately analysed, as the quantitative analysis of cholesterin is very much more difficult in practice than it is in theory, since the entire saponification of the neutral fats, so as to separate them thoroughly from cholesterin, is a tedious matter and requires frequent repetition if one desires to get really accurate results. In those few cases in which the analysis of cholesterin was carried out, the quantity found was so extremely small that it in no way interfered with the general results obtained if the entire extract was regarded as neutral fat.

The Splitting up of Fat in various parts of the Alimentary Canal in Normal Dogs on a Milk Diet.

The following are the results obtained in normal dogs :—

Table I.—Normal Dog, weighed 6·66 kilos., killed by Chloroform Seven Hours after being fed with Milk.

	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soaps.	
	Total.	p. c.	Total.	p. c.	Total.	p. c.	Total.	p. c.
Given.....	9·471	100	9·456	99·85	0·012	0·12	0·003	0·03
Found in—								
Stomach.....	3·503	100	2·691	76·82	0·768	21·92	0·044	1·26
Small intestine.	0·845	100	0·214	25·33	0·600	71·01	0·031	3·66
Large intestine.	0·791	100	0·296	37·42	0·445	56·26	0·050	6·32

In Table I it is seen that the total quantity of fat given is diminished in the stomach to almost a third during the seven hours of digestion, so that of the 9·471 grams of fat that had been given in the milk only 3·503 grams were found, the rest having passed through the pylorus. The composition of this fat is, however, entirely changed, for while the milk fat contained 99·85 per cent. as neutral fat, that in the stomach was only 76·82 per cent.

We see, further, that the neutral fats have disappeared from the stomach more rapidly than the others as, although 9·456 grams of neutral fat were given, only 2·691 grams remained.

The formation of free fat acids is very marked, for whereas the milk only contained 0·12 per cent. the stomach contained as much as 21·92 per cent. This speaks strongly in favour of Ogata and Marcet's view that neutral fat can be broken up into free fat acids in the stomach, unless one is to believe that the fat acids had been retained in the stomach, and that the increased percentage was obtained by some condition allowing the passage of the neutral fats and not the fat acids from the stomach. But it may further be seen that this would not account for the increase in the free fat acids, as the total quantity of fat acids given was only 0·012 while the stomach contained no less than 0·768. It may, therefore, be regarded as conclusively established that the excess of fat acids was formed from the neutral fat of the milk in the stomach itself.

More remarkable is the proportion of fat acids combined as soaps. For the milk itself contained only 0·03 per cent. of fat acids in the form of soap, while the stomach contained 1·26 per cent. That there had been an absolute formation of soap and not any increase

by the mere passage of the neutral fat from the stomach is proved by the fact that the total quantity of such soaps given was only 0·003, while the stomach at the end of seven hours contained 0·044 gram.

It is thus shown beyond question that the stomach is not only capable of breaking up neutral fat into free fat acids and glycerine, but that some of these free fat acids are able to find in the gastric secretion an alkaline substance with which they can form soaps even in the acid stomach contents.

Turning next to the contents of the small intestine, we find that the total fat is very small in quantity, there only being 0·845 gram, of which 0·214 gram remain as neutral fat, so that only 25·33 per cent. of the total fat remains in the small intestine as neutral fat. Fat acids, on the other hand, are very greatly increased, 71·01 per cent. of the total fat being present as fat acids. The absolute quantity of fat acids present in the small intestine is only 0·600 gram; that is to say, really less than was present in the stomach. The percentage proportion of the soaps is also increased over that of the stomach, being 3·66 per cent., although the total quantity present is somewhat less than what was present in the stomach, being only 0·031 gram.

In the large intestine we find that the total fat is only slightly less than that present in the small intestine, being 0·791 gram. This is composed of 0·296 gram of neutral fat, that is to say, somewhat more than was present in the small intestine, and making the percentage of neutral fat present in the large intestine as much as 37·42. The free fat acids are only 0·445 gram, and the percentage is also decreased to only 56·26 per cent.; that is to say, the percentage of fat acids present in the large intestine is greater than that present in the stomach, but not so large as that present in the small intestine.

The soaps, on the other hand, are increased, being 0·050 gram, and constitute a percentage of 6·32; so that the amount of soap present in the large intestine is almost double that present in the small intestine and five times that in the stomach.

In this the second normal animal (Table II) as much as 43·13 per cent. of the total fat given had left the stomach during the seven hours' digestion, so that the stomach only contained 12·970 grams of the 22·806 grams of fat given in the milk.

On examining the composition of the total fat present in the stomach we find that it has undergone a marked change, since, of the milk fat given, no less than 96·69 per cent. was present as neutral fat, while the stomach contained 81·12 per cent., the total neutral fat having diminished from the 22·05 grams given in the milk to only 10·52 grams. Hence 15·57 per cent. of the neutral fat given had been changed into some other form during its seven hours' sojourn in the stomach.

Table II.—Normal Dog, weighed 12·825 kilos., killed by Chloroform Seven Hours after feeding with Milk.

	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soaps.	
	Total.	p.c.	Total.	p. c.	Total.	p. c.	Total.	p. c.
Given.....	22·806	100	22·050	96·69	0·744	3·25	0·012	0·06
Found in—								
Stomach	12·970	100	10·522	81·12	2·436	18·78	0·012	0·10
Small intestine.	0·977	100	0·349	35·85	0·608	62·19	0·020	2·06
Large intestine.	0·402	100	—	—	—	—	—	—

As regards the free fat acids, the milk given contained 0·744 gram, *i.e.*, 3·25 per cent. of the total ether extract was obtained as fat acid, which is far more than usual, and is probably in part due to the fact that the milk stood in the laboratory for some time previously to its being boiled for the experiment.

The stomach contained 2·436 grams of fat acids, *i.e.*, 18·78 per cent. of the total fat was present in the stomach as free fat acids. The percentage of the neutral fat had diminished, while the percentage of the fat acids had increased. The fat acids, combined as soaps in the stomach, remained exactly the same as the quantity originally given, but since the percentage had increased from 0·06 to 0·10, it is not at all likely that that alone of the components of the total fat had wholly remained in the stomach, so that we may consider that this case is no exception, and that the stomach formed not only fat acids but also soap.

In the small intestine only 0·977 gram of fat was found, and since no less than 9·836 grams had passed through the pylorus during the period of digestion, either 8·859 grams had passed on into the large intestine or had been absorbed. The total fat contained only 0·349 gram as neutral fat, so that the percentage had fallen to 35·85 per cent. (the milk given contained 96·69 per cent.). The free fat acids amounted to 0·608 gram, that is 62·19 per cent. of the total fat.

As originally the milk only contained 3·25 per cent. of fat acids, we can conclude that no less than 58·94 per cent. of the neutral fat had been converted into fat acids in its passage as far as the cæcum. The fat acids present as soaps had increased to 0·02 gram, which raises the percentage of fat acids present as soaps to no less than 2·06, an amount which closely resembles that present in the foregoing case.

In the large intestine we unfortunately cannot follow the changes in this experiment, as the neutral fat and free fat acids and soaps

were lost in the process of separation. We, however, see that only 0·402 gram of the total ether extract was left in the large intestine, so that 8·457 grams of the fat had really been absorbed during the seven hours' digestion; that is to say, as much as 85·98 per cent. of the total quantity of fat that had traversed the pylorus.

Table III.—Normal Dog, weighed 10·35 kilos., killed by Chloroform Seven Hours after feeding with Milk.

	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soaps.	
	Total.	p. c.	Total.	p. c.	Total.	p. c.	Total.	p. c.
Given.....	12·027	100	11·533	95·89	0·455	3·78	0·039	0·33
Found in—								
Stomach.....	8·116	100	6·061	74·67	2·012	24·81	0·043	0·52
Small intestine.	0·623	100	0·080	12·84	0·522	83·46	0·021	3·70
Large intestine.	0·741	100	0·229	30·91	0·452	61·03	0·059	8·06

In Table III the total fat given was 12·027 grams, and after seven hours' digestion 8·116 grams remained in the stomach. The stomach had consequently discharged 3·911 grams of the fat into the intestines (*i.e.*, 32·52 per cent.).

The fat that remained in the stomach during the seven hours had undergone similar changes to those already described in the former two experiments. The neutral fat had decreased from 11·533 grams in the milk given to 6·061 grams. Thus, while the milk fat contained 95·89 per cent. as neutral fat, the stomach only contained 74·67 per cent., so that at least 21·22 per cent. of the neutral fat given had been split up in the stomach into free fat acids or traversed the pylorus.

The free fat acids on the other hand had increased very markedly; for whereas the milk contained but 0·455 gram the stomach contained 2·012 grams, the percentage composition having thus changed from 3·78 to 24·81.

The soap had also increased in quantity in the stomach from 0·039 gram, in the milk given, to 0·043 gram, which altered the percentage composition from 0·33 to 0·52 per cent.

Thus, in all three experiments we have confirmation of the fact that neutral fat is split up into free fat acids while in the stomach itself, and that some of these free fat acids formed soaps.

With regard to the small intestines we find only 0·623 gram of ether extract was present, while we know that 3·91 grams had

passed through the pylorus during this time. Of the total fat only 0·080 gram was found as neutral fat, so that its percentage proportion amounted to only 12·84 per cent., no less than 61·83 per cent. of the neutral fat having been broken up since it traversed the pylorus.

The free fat acids increased to 0·522 gram, therefore the percentage of the total fat present as fat acids was 83·46. As originally only 3·78 per cent. of the total fat was in the form of free fat acids, therefore 79·68 per cent. of the neutral fat has been broken up into free fat acids in its passage along the stomach and small intestine, and as the stomach contained 24·81 per cent., therefore 55·87 per cent. of the free fat acids had been formed in the small intestine.

The quantity of fat acids present as soap in the small intestine was only 0·021 gram, but the total quantity of fat being so small the percentage was 3·70.

It would thus appear as if 2·37 per cent. of the ether extract had been converted into soap in its passage from the mouth to the cæcum, and since 0·19 per cent. was found in the stomach, 2·18 per cent. had been formed in the small intestine.

The large intestine contained 0·741 gram of total fat, of which 0·229 gram remained in the form of neutral fat, and the percentage of the neutral fat was 30·91, *i.e.*, somewhat higher than that found in the small intestine, although less than half the percentage in the stomach. The free fat acids left in the large intestine were 0·452 gram, *i.e.*, 61·03 per cent. of the total ether extract present, the quantity is therefore less than is present in the small intestine, but higher than that in the stomach.

The fat acids combined as soap in the large intestine were 0·059 gram, so that the percentage was 8·06 of the total fat.

With regard to the absorption of fat, in this case we see that where 12·027 grams were given only 9·479 were regained after seven hours' digestion, so that at least 2·548 grams had been absorbed, *i.e.*, 21 per cent. of the total given during the seven hours' digestion. However, as no fat is probably absorbed from the stomach, we ought only to calculate the percentage absorption from the 3·911 grams which had passed into the small intestine, *i.e.*, 65·16 per cent. of this had been absorbed.

The Splitting up of Fat in various parts of the Alimentary Canal in Dogs on a Milk Diet and in which the Pancreas had been removed.

Having shown the manner in which fat is broken down in the intestines of dogs under normal circumstances, we can now consider the changes that occur in the fats when milk is given to dogs in which the pancreas has been previously removed.

In the following experiments, as has been already said, the pancreas was removed on the second day of fasting, and the animals allowed to fast still further two days after the operation, the rectum being washed out with copious warm water enemas, as by this means it was hoped to rid the intestines of any residue of pancreatic secretion which might otherwise be present, and at the same time that the washing out would diminish the bacteria of the large intestine to a considerable extent.

In the four following dogs in no case was there, by analysis, any evident absorption of fat from the intestines during the space of four to seven hours. At the same time, the fact that the quantity of fat found equalled or surpassed the quantity of fat given at the end of this time, is no definite proof that no absorption of fat took place from the large intestine. In all probability there was some absorption, as the lymphatics shone out as white lines and had not the appearance of those normally present in fasting animals, though they were not so prominent or filled as was the case in normal dogs during the same period of digestion. The remarkable fact that the quantity of fat was equal to, or in excess of, that given will appear to be explained by the character of the intestinal excretion, or so-called secretion, which has now been practically demonstrated to be always present by Hermann, Ehrenthal, Berenstein, and Fr. Voit, which is in all probability a substance rich in fat and very much more profuse than we are ordinarily led to believe.

Table IV.—Weight, 6·19 kilos. Total Extirpation of Pancreas. Dog being given Milk forty-eight hours after Removal of Gland and killed by Chloroform four hours after the Diet.

	Total ether extract.		Neutral fat.		Free fat acid.		Fat acids as soap.	
	Total.	p. c.	Total.	p. c.	Total.	p. c.	Total.	p. c.
Given.....	7·602	100	7·350	96·68	0·248	3·26	0·004	0·06
Found in—								
Stomach.....	7·406	100	4·513	60·94	2·727	36·82	0·166	2·24
Small intestine.	1·456	100	0·820	31·17	0·452	56·55	0·184	12·48
Large intestine.	0·444	100	0·159	35·81	0·271	61·04	0·014	3·15

In Table IV the total quantity of fat given was 7·602 grams, and after the period of four hours, no less than 7·406 grams still remained in the stomach, so that during four hours only 0·196 gram had entered the intestines, or but 2·58 per cent. of the quantity

given had passed through the pylorus. Further, since the small and large intestines contained no less than 1·902 grams, while only 0·196 gram of this had entered from the pylorus, at least 1·704 grams of this fat must be regarded as derived directly from the intestinal secretion and not from the fat administered in the food. Of course, in this case, the fat might be partly derived from the bile, as, although the pancreas had been entirely removed, the bile-ducts had been in no way interfered with. The fact that the dog had not only fasted for four days but that it had had its bowels washed out daily, would leave little doubt that this fat could not be residues of fat given five days previously.

In investigating the various changes that the fat underwent during its sojourn in the stomach, only 4·513 grams were found as neutral fat; that is to say, 60·94 per cent. of the total fat still remained as neutral fat. The milk contained, on the other hand, 96·68 per cent. as neutral fat, so that no less than 25·74 of the fat given had been split up during its four hours' sojourn in the stomach into free fat acids and glycerine. The fat acids were increased to 2·727 grams, and this was no doubt due to the splitting-up of the neutral fat in the stomach, since only 0·248 gram fat acids were given in the diet. The percentage of fat acids present in the stomach being 36·82, the quantity found in the viscus was ten times the amount present in the milk given.

As regards the soap, it was increased to 0·166 gram, so that the percentage composition of the fat acids combined as soaps was 2·24 as compared to the 0·06 per cent. of fat acids (combined as soaps) observed in the milk.

From these facts it is plain that even after the removal of the pancreas the stomach, as in normal dogs, is capable of converting a considerable quantity of the neutral fat into fatty acids, and still further forming soaps.

The small intestine contained 1·456 grams of fat, and, as has already been said, most of this was not derived from the milk given but partly from the excretion of the intestines; however, it will be as well to examine its chemical composition in the same way as we have done in the normal dogs.

Of this fat, 0·820 gram was found to be neutral fat, so that the percentage of neutral fat was 31·17. The free fat acids were 0·452 gram, the total quantity being increased to 56·55 per cent., and the soap had increased to 12·48 per cent., the total quantity really being 0·184 gram.

We see that the quantity of fat acids had very much increased in the small intestine over that found in the stomach, even although no pancreatic secretion was present.

In the large intestine the total quantity of fat present was only

0·444 gram, and of this 0·159 gram only remained as neutral fat. The free fatty acids were only 0·271 gram, the percentage being no less than 61·04, showing that the percentage of fat acids was greater in the large intestine than either in the small intestine or in the stomach.

The soaps had fallen to only 0·014 gram, or 3·15 per cent., so that they were really smaller than the quantity in the large intestine, but larger than that in the stomach.

Table V.—Weight, 6·98 kilos. Total Extirpation of the Pancreas. Milk Diet forty-eight hours after Removal of the Gland, and killed by means of Chloroform six hours later.

	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soap.	
	Total.	p. c.	Total.	p. c.	Total.	p. c.	Total.	p. c.
Given.....	8·496	100	7·134	83·98	1·352	15·91	0·008	0·09
Found in—								
Stomach	7·624	100	4·964	65·11	2·573	33·75	0·087	1·14
Small intestine.	0·822	100	0·312	37·95	0·452	54·99	0·058	7·06
Large intestine.	0·251	100	0·082	32·68	0·113	45·01	0·056	22·31

In Table V the animal was allowed to digest for six hours after the feeding. In this case the total fat given was 8·496 grams, while after six hours' digestion the stomach still contained 7·624 grams; hence only 0·872 gram of the fat had passed through the pylorus into the intestines in this space of time.

In the small and large intestines 1·073 grams were collected, so that 0·201 of this must have been due to the excretion from the intestinal wall or derived from the bile.

The stomach contained only 4·964 grams of its fat as neutral fat, and whereas the milk given contained 83·98 per cent. of its fat as neutral fat, the stomach only contained 65·11 per cent. This was due to the increase of free fat acids for the most part, but also to some extent to the formation of soaps. The free fat acids present in the stomach were 2·573 grams, while the original milk only contained in all 1·352 grams, therefore 33·75 per cent. of the total fat was present in the stomach as free fat acids.

The soaps also had increased in quantity. The stomach contained 0·087 gram, while the milk given only contained 0·008, the percentage of fat acids present as soaps being thus raised to 1·14.

It is clearly brought out in this experiment that there was a very

great delay in the quantity of fat leaving the stomach during the six hours, as compared with what was observed in the normal dogs.

In the small intestine the total fat present was only 0·822 gram. This is somewhat less than what we know has already passed through the pylorus, for it was seen that 0·872 gram of fat had left the stomach during the six hours. The neutral fat in the intestine was only 0·312 gram, so that the quantity had diminished to only 37·95 per cent., a little more than half of that present in the stomach.

The quantity of free fat acids was 0·452 gram, the percentage having increased to 54·99, almost double what was present in the stomach. The fat acids present as soaps were 0·058 gram, bringing the percentage composition of the soap to 7·06.

The large intestine contained 0·251 gram of fat; of this, a very great part must be attributed to the intestinal excretion, and was certainly not residue of the fat given. The neutral fat is only 0·082 gram, that is to say, 32·68 per cent. of the total fat only remains as neutral fat when it has reached as far as the large intestine.

The free fat acids amounted to 0·113 gram, so that the percentage proportion was 45·01, this being less than that present in the small intestine.

The soap present was 0·056 gram, the total quantity being almost the same as that present in the small intestine, but as the quantity of total fat is very much smaller, the relative amount of fat acids combined as soaps reaches in the large bowel a very much higher point than in any other part of the alimentary canal, being 22·31.

In the next two dogs in which the pancreas was removed the period of digestion allowed was seven hours in each case.

Table VI.—Weight, 5·40 kilos. Total Extirpation of the Pancreas. Milk given forty-eight hours after Removal of the Gland. Killed by Chloroform after seven hours' digestion.

	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soap.	
	Total.	p. c.	Total.	p. c.	Total.	p. c.	Total.	p. c.
Given.....	7·191	100	6·877	95·63	0·309	4·30	0·005	0·07
Found in—								
Stomach.....	6·554	100	2·708	41·32	3·828	58·41	0·018	0·27
Small intestine.	1·077	100	0·470	43·64	0·561	52·09	0·046	4·27
Large intestine.	0·644	100	0·175	27·17	0·359	55·75	0·110	17·08

In the above Table VI, of 7·191 grams of fat given no less than 6·554 still remained in the stomach, so that only 0·637 gram had

traversed the pylorus. The small and large intestines together contained no less than 1·721 grams of fat, and therefore 1·084 grams of the fat had been excreted from the intestines or liver during the period of seven hours. This appears an enormous quantity when we compare it with the small amount in the case of Table V, in which only 0·201 gram was excreted in six hours.

The stomach contained 2·708 grams of neutral fat remaining from the 6·877 grams of the neutral fat given in the milk. Thus the percentage of neutral fat found was 41·32, while the neutral fat in the milk given was no less than 95·63; hence 58·41 per cent. of the neutral fat given had been split up into fat acids during seven hours in the stomach.

The quantity of free fat acids was 3·828 grams, and the soaps had increased to 0·018 gram, this constituting a percentage of 0·27.

The small intestine gave as the percentage of neutral fat 43·64, as compared with 41·42 present in the stomach.

This is a very different thing to what is found in the case of the normal dogs, where the fat acids in the small intestine were greatly increased and the neutral fat much diminished, for here the free fat acids were only 52·09 per cent., *i.e.*, very slightly above the amount of neutral fat present in the small intestine, and less than the fat acids present in the stomach. The small intestine also contained no less than 4·27 per cent. of fat acids in the form of soap.

In the case of the large intestine the total quantity of fat present was 0·644; of this, 0·175 still remained as neutral fat, the percentage being 27·17, while 55·75 per cent. was present in the form of free fat acids, and 17·08 per cent. of the fat acids had combined to form soaps.

Table VII.—Weight, 6·70 kilos. Total Extirpation of the Pancreas. Milk given forty-eight hours after Removal of the Gland, and killed by Chloroform after seven hours' digestion.

	Total ether extract.		Neutral fat.		Free fat acids.		Fat acids as soap.	
	Total.	p. c.	Total.	p. c.	Total.	p. c.	Total.	p. c.
Given.....	12·027	100	11·533	95·90	0·455	3·78	0·039	0·32
Found in—								
Stomach.....	9·358	100	9·177	98·07	0·159	1·70	0·022	0·23
Small intestine.	0·464	100	0·075	16·17	0·361	77·79	0·028	6·04
Large intestine.	1·672	100	0·668	39·95	0·990	59·21	0·014	0·84

In Table VII the dog received a larger quantity of fat than usual, and whether in consequence of this or through some other

reason, there was, towards the end of the seven hours' digestion diarrhœa which necessitated the collecting of some of the fæces passed, and by reason of this there was a possibility of some slight loss.

This case alone, of all the dogs in which the pancreas was removed, indicated any absorption of fat, for out of the 12·027 grams of the fat given 0·533 gram was not recovered. It was, however, perfectly obvious that in this experiment this deficit was attributable to loss in collection of the fæces, although everything to hinder such an error was attempted.

The stomach contained 9·358 grams of fat, of this no less than 9·177 grams was in the form of neutral fat. In fact, 98·07 per cent. of the total fat was present in this case as neutral fat, while the milk given contained only 95·90 per cent.

The free fat acids in the milk given contained 0·455 gram, while in the stomach only 0·159 gram was found; that is to say, 1·70 per cent. of the total fat only was present as fat acids in the stomach. Soaps also were 0·022 gram, and the percentage 0·023.

In this dog alone, of either the normal dogs or those in which the pancreas was removed, was there no indication of breaking up of neutral fat into free fat acids in the stomach, as far as the total quantity of fat acids found in the stomach was concerned. The explanation of this is very difficult. It may be due to the fact that the milk fat in this case had very considerably broken up before it was administered, the milk being somewhat acid, and it had been boiled down considerably in order to decrease the volume given. The quantity of fat which had left the stomach during the seven hours' digestion was no less than 22·19 per cent. of the total given, that is, much more than occurred in any of the other dogs in which the pancreas had been previously removed. In fact, this out-flow is more like the normal dogs. It is, therefore, possible that the soaps and fat acids which had been formed in the stomach had to a considerable extent left it, and if to a greater extent than the neutral fat had thus caused the percentage of neutral fat to be higher than that given.

The quantity of fat in the large gut was more than in the small intestine, and this also was exceptional, as in all the other cases the reverse was found after seven hours, and it can be only explained by the fact that on account of the milk being sour and the diarrhœa which occurred, there was a hurrying along of the contents of the alimentary canal.

The quantity of fat in the small intestine was only 0·464 gram, of which 0·075 gram was present as neutral fat; that is to say, only 16·17 per cent. of the total fat was present in the stomach as neutral fat. The fat acids on the other hand were no less than 0·361 gram, that is 77·79 per cent. of the total fat present in the small intestine.

This result may not be due entirely to the breaking up of the fat in the small intestine, but to the increased out-put of fat acids from the stomach. The soaps equalled 6·04 per cent. of the total fat present in the small intestine, although the actual quantity was only 0·028 gram.

In the large intestine the quantity given in the table was not only that which was present in the intestine after death, but also that which was collected in the fæces, passed during the last hour or so of life. It contained 1·672 grams of fat, of which 0·668 gram were in the form of neutral fat, and 0·990 had been broken up into free fat acids. So that 59·21 per cent. of the total fat was in the form of fat acids, and 39·95 per cent. only as neutral fat.

The soaps were not very much increased, the total quantity being only 0·014, that is to say 0·84 per cent.

Summary.

When we come to compare the results found in the normal dogs during the absorption of milk fat with that in the dogs in which the pancreas has been removed, we see that there is not such a great difference as one would be led to suspect by the old teaching of the fat splitting action of the pancreas.

For the purpose of comparison it will be better to tabulate the average results of the preceding experiments so that the normal results may be more readily compared with those in which the pancreas had been previously removed. In this way it is easy to detect any real difference in the percentage quantities in the alimentary canal.

Table VIII.—Comparing the Percentage Composition of the Fats in the Different Parts of the Alimentary Canal on a Milk Diet, of Normal Dogs (A) with those in which the Pancreas (B) had been previously removed.

	Neutral fat.		Free fat acids.		Fat acids as soap.	
	Normal. A.	Pancreas. B.	Normal. A.	Pancreas. B.	Normal. A.	Pancreas. B.
Stomach.....	77·54	68·17	18·50	31·29	0·63	0·55
Small intestine....	24·67	32·59	72·22	61·62	3·14	5·79
Large intestine....	34·17	33·27	58·65	53·32	7·19	13·41

The average number of cases in the above Table VIII is three in each column, except in that of the large intestine of the normal dogs,

in which the results of only two experiments were used, and the period of digestion was seven hours, except in one case of removal of the pancreas in which it was only six hours.

As far as the stomach is concerned, the table shows that there is a slight increase in the quantity of fat acids in dogs in which the pancreas has been removed, as the percentage noted was 31·29 per cent. of the total fat, while in the normal dogs which were employed as control, only 18·50 per cent. of the total fat was present in the form of free fat acids. This would be in favour of a greater fat splitting-up activity of the stomach in dogs in which the pancreas had previously been removed. That a greater splitting-up action was really the cause of this increase is difficult to believe. It would seem much more likely that the increased percentage of fat acid in the dogs without the pancreas was due to the greater retention of fat in the stomach, for in all cases in which the pancreas had been removed, the expulsion of fat from the stomach was very much delayed.

The percentage quantity of fat acids present as soap in both the normal and the pathological dogs is practically equal, for in both cases no less than 0·63 and 0·55 per cent. of the free fat acids were present as soap in the stomach.

The table therefore proves that both in normal dogs as well as in those in which the pancreas has been removed, the stomach is capable of not only splitting up neutral fat into free fat acids and glycerine, but further, that the free fat acids are capable of finding alkalis in the stomach itself wherewith to form soaps. The power of the free fat acids forming soap is, however, extremely limited in the stomach.

The principal fat-splitting action really begins, not in the stomcab, but after it has left the pylorus. That this should be the case in normal dogs has long been held by all physiologists, and was considered due to the action of the pancreas, and the degree to which this occurs is shown in the table, according to which in the normal dog at least 72·22 per cent. of the fat present in the small intestine was in the form of free fat acids, only 24·67 per cent. being present as neutral fat. But also in the case where the pancreas had been entirely extirpated, so that no pancreatic juice could possibly reach the intestine, and the fasting had been sufficiently long to remove any pancreatic ferments which might have been previously present in the intestines, we still find a large increase in the quantity of the fat acids present, the fat acids being no less than 61·62 per cent. of the total fat. There can be no doubt, therefore, that even when no pancreatic secretion has reached the intestines, a very considerable quantity of the neutral fat is split up into free fat acids in the small intestine, although the quantity there formed is not so great as when the pancreatic secretion has been able to add its share of work.

The free fat acids combined with soaps in the normal dog were only 3·14 per cent. of the total fat present, while those dogs in which the pancreas had previously been removed had no less than 5·79 per cent.

This is a most interesting fact, for in spite of the absence of the alkaline pancreatic secretion, it is found that not only nearly the normal quantity of free fat acids are present, but a considerable increase of fat acids have formed soaps, so that the intestines have been able to secrete a sufficiency of alkali substances to form soaps.

The contents of the large intestine in the second group of cases resemble each other far more closely. In fact, in the case of the free fat acids, the quantity present in both the normal control dogs and those in which the pancreas had previously been removed is, for all practical purposes, equal, the control dogs having 58·65 per cent. of fat, in the form of free fat acid, while those dogs in which no pancreatic secretion had reached the intestines, 53·32 per cent. The effect of the want of pancreatic secretion, although exhibited to a small extent in the small intestine, and causing a diminution in the quantity of fat acids present, was no longer apparent in the large intestine.

The quantity of fat acids present as soaps in the large intestine varies, although not to a greater extent than was the case in the small intestine. Those dogs in which the pancreas had been previously removed contained 13·41 per cent. of the total fat in the form of fat acids combined as soaps, while 7·19 per cent. only was in this form in those dogs which were used as control experiments.

May 13, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates recommended by the Council for election into the Society were read as follows:—

Bell, Robert, LL.D.
Broadbent, Sir William Henry, Bart.
Chree, Charles, M.A.
Elwes, Henry John, F.L.S.
Haldane, John Scott, M.D.
Haswell, Professor William A., D.Sc.
Howes, Professor George Bond, F.L.S.
Kipping, F. Stanley, D.Sc.
Mathews, George Ballard, M.A.
Murray, George Robert Milne, F.L.S.
Neville, Francis Henry, M.A.
Nicholson, Professor H. Alleyne, M.D.
Thomson, Professor John Millar, F.C.S.
Trouton, Professor Frederick Thomas, M.A.
Turner, Professor Herbert Hall, M.A.

The following Papers were read:—

I. "An Attempt to cause Helium or Argon to pass through Red-hot Palladium, Platinum, or Iron." By WILLIAM RAMSAY, F.R.S., and MORRIS W. TRAVERS.

II. "On the Negative After-images following brief Retinal Excitation." By SHELFORD BIDWELL, M.A., LL.B., F.R.S.

III. "A Dynamical Theory of the Electric and Luminiferous Medium. Part III. Relations with Material Media." By JOSEPH LARMOR, F.R.S., Fellow of St. John's College, Cambridge.

IV. "On a new Method of Determining the Vapour Pressures of Solutions." By E. B. H. WADE, B.A., Scholar and Coutts-Trotter Student of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S.

V. "On the Passage of Heat between Metal Surfaces and Liquids in Contact with them." By T. E. STANTON, M.Sc. Communicated by Professor OSBORNE REYNOLDS, F.R.S.

VI. "On the Magnetisation Limit of Iron." By HENRY WILDE, F.R.S.

"An Attempt to cause Helium or Argon to pass through Red-hot Palladium, Platinum, or Iron." By WILLIAM RAMSAY, F.R.S., and MORRIS W. TRAVERS. Received April 9,—Read May 13, 1897.

To chronicle experiments which produce no result is a necessity, although not entirely an agreeable one. Whatever the reason of the passage of hydrogen through red-hot iron, and through moderately heated palladium, and platinum—whether it be due to the solubility of the gas in the metal, or to the formation of an easily decomposable compound—neither argon nor helium is able to pass through any one of these metals, even at a fairly high temperature. This would imply their inability to form any compound, however unstable, with these metals, or to dissolve in them at a red heat. Such inactivity is in accordance with their general behaviour, and is still another proof of their inertness.

The experiment was made in the following manner:—A tube of hard, infusible glass was connected at one end with the reservoir of the gas under experiment, helium or argon. Into its other end was corked a tube of platinum, closed with a palladium cap, or, if iron was the metal under experiment, with a tube of thin wrought iron, also closed at the end; the closed end of the interior tube was placed so that it could be raised to a bright red heat by bringing a blow-pipe flame to bear on the hard glass tube. The open end of the metal tube was cemented to a glass tube, attached to a Töpler's pump, and provided with a Plücker's vacuum tube, so that the spectrum of any gas passing through the metal could be observed. This afforded, at the same time, a most delicate test of the presence of the gas under experiment. The metal tube was exhausted, until green phosphorescence appeared in the vacuum tube, and the gas, helium or argon, was admitted into the space between the glass and the metal tube, at atmospheric pressure. The glass tube was then heated to the highest temperature attainable with a blow-pipe—perhaps 900° or 950° C. In no case, whether the metal tube consisted of palladium, platinum, or iron, was there the smallest transpiration of gas, even after half an hour. The phosphorescent vacuum remained in all experiments quite unimpaired.

"On the Negative After-images following Brief Retinal Excitation." By SHELFORD BIDWELL, M.A., LL.B., F.R.S. Received April 15,—Read May 13, 1897.

The experiments to be described were undertaken with the object of ascertaining the true nature of certain phenomena referred to in a recent communication, which had been left over for further examination. For full details reference should be made to the former paper,* but it will be convenient to recapitulate very shortly one or two of the results which were arrived at.

It was found that if the image of a bright object upon a dark background were suddenly formed upon the retina, the object generally appeared to be surrounded for about one-tenth of a second by a narrow red border. As a consequence of this circumstance, a black line drawn upon white paper appeared for a moment after it was first exposed to view to be entirely red. Methods were described by which these coloured borders could be seen continuously for any length of time, so that, for example, a design in black lines appeared as if drawn with red ink. The red border was explained by supposing a momentary sympathetic affection of the red nerve-fibres just outside the boundary of the luminous patch projected upon the retina. But, in some experiments, it was noticed that, when the intensity of the illumination was increased beyond a certain limit, the colour of the transient border was modified. Under very strong illumination the colour of the border was greenish-blue; sometimes (as in Experiment II, p. 371) it was of two colours—greenish-blue within and red without. It was with regard to the cause of these greenish-blue and particoloured borders that I then considered it preferable to "refrain from any speculation."

It has now been ascertained, almost beyond doubt, that the greenish-blue colour is due simply to the negative after-image of the red, this after-image being in certain cases so much more conspicuous than the red band which originates it, that the latter altogether escapes notice. The experiments upon which this conclusion is based gave results of a remarkable and unexpected character; there is no record, so far as I am aware, that anything of the kind has been previously noticed.

It should be stated that the negative after-images here referred to are of the kind which appear upon a white ground after the gaze has been directed for a time upon a coloured object. The tints of such after-images are generally said to be always complementary to colours of the objects which excite them; they are, however, rather weak or impure, being largely diluted with white light.

* 'Roy. Soc. Proc.,' 1896, vol. 60, p. 368.

Apparatus.

The only apparatus employed was a rotating disk of which one-half of the surface was black and the rest white, a sector of 45° being cut out at the junction of the black and white portions. The disk, which was 8 inches (20 cm.) in diameter, was made of tin-plate; a piece of thick brass wire was fixed along the arc bounding the open sector to restore the balance. Half the disk was covered with black velvet, for no pigment among several that were tried appeared sufficiently black in a strong light; the remaining portion of the surface was coated with white paper. The disk was turned in such a direction that the open sector was preceded by black and followed by white. It was usually illuminated by an electric lamp of 25-candle power with a ground glass bulb, the intensity of the illumination being regulated by varying the distance of the lamp; sometimes sunlight or diffused daylight was employed. The objects under examination were placed on the other side of the disk and viewed intermittently through the open sector.

Experiment I.

A card with black letters printed upon it was placed behind the disk, which was illuminated in front by the lamp at a distance of 2 feet (60 cm.). When the disk made about six turns per second, the black letters appeared to be bright red; this was of course so far merely a repetition of the old observation. The lamp was then gradually brought nearer to the disk, and as it approached the aspect of the letters underwent a change. At first they exhibited a peculiar shimmer, independently described by a great number of persons as either a "silvery," "lustrous," or "metallic" appearance; it was plainly due to flashes across the letters of some tint much lighter than the red, by which they were still mainly characterised. When the distance of the lamp was diminished to about 4 inches (10 cm.), all trace of the red had disappeared, and the letters assumed a luminous greenish-blue tint.

Experiment II.

A card was substituted upon which were printed words and figures in both black and red. It was found possible, by suitably adjusting the intensity of the light and the speed of rotation, to make such a perfect match between the letters which were actually red and those which only appeared so, that an unpractised observer could not distinguish between them. When the lamp was placed near the disk, all the letters, both black and red, appeared greenish-blue. The greenish-blue tint of the actually red letters was, moreover, more

intense or saturated than that acquired by the black ones. Bright diffused daylight produced nearly the same effect as the electric lamp at 4 inches.

These two experiments together tend to show that, whatever may be the cause of the subjective red border, the nerves of the retina when under its influence behave very much in the same way as if excited by an external red object.

Experiment III.

It was remarked in the former paper (p. 376) that the greenish-blue tint appeared to be of the hue that is complementary to red. Consideration of this fact, in conjunction with the others that have been mentioned, suggested the question whether this tint might not be referable to a negative after-image of the red border. At first sight such a supposition appeared to be an exceedingly improbable one. It is known that under ordinary circumstances a conspicuous after-image can be seen only after prolonged staring at the coloured object, and it seemed incredible that after an excitation lasting for not more than about one-fiftieth of a second there should be developed an after-image of such intensity as to completely overwhelm the primary impression, so that the latter was altogether unnoticed. When, however, the question was tested by experiment, the results were found to be consistent with the conjecture.

Several cards, measuring about 5 inches by 3, were covered with paper and silk of different colours, and observed through the opening in the rotating disk while the 25-candle power lamp was at a distance of about 6 inches. In every case the original hue entirely vanished, and was replaced by a pale tint of the complementary colour. In order to assist in the recognition of these tints, it was found useful to cover the cards with two different colours, which were approximately complementary to each other. Thus, for example, one-half of a card was covered with red silk, and the other half with green. Under the rotating disk the red portion becomes pale greenish-blue, and the green portion rose-pink, these two hues being recognized by an inexperienced person without the slightest hesitation. The immediate reversal of the colours which takes place when the disk is suddenly stopped is very striking.

It should be remarked that there is no blurring of the image upon the retina when objects are viewed through the rotating disk; even the texture of the coloured silk fabrics is seen quite distinctly. The most suitable speed of rotation is not exactly the same for all persons; the disk should generally be made to turn some six or eight times per second.

Experiment IV.

What may be regarded as the negative after-image of a black spot upon a white ground is shown by the ordinary method of observation as a patch of more intense whiteness. It was of interest to ascertain whether, by means of the disk arrangement, a black object could be made to assume a continuous appearance of abnormal whiteness.

Several broad bands $\frac{3}{8}$ inch wide were painted with "artist's black" upon a white card. When this was placed behind the rotating disk and exposed to direct sunshine, the effect was exceedingly striking, the painted portions appearing as bands of glittering whiteness upon a ground which by contrast seemed to be of a neutral grey tint. With strong lamplight or diffused daylight the phenomenon was almost equally remarkable; but, for some unexplained reason, the ground seemed to acquire a very pale purple tint instead of being, as in the former case, quite neutral.

All the above observations can, of course, be accounted for quite easily in the ordinary way by the Young-Helmholtz theory of vision. The only novelty in connection with them consists in the demonstration of the fact that the action of light after a period of darkness (which need not exceed one-twentieth of a second) can appreciably diminish the sensibility of the retinal nerve-fibres in a space of time so extremely short that if the light be coloured its colour is not consciously perceived.

The experiments clearly indicate the origin of the greenish-blue borders which I was unable to explain at the time when my former paper was written. If the hypothesis suggested in that paper is correct, it now seems that the coloured borders produced by sudden changes of illumination are, whatever their hue, in all cases primarily due to sympathetic action of the red nerve-fibres of the retina.

Note added May 6.

[The transient blue border which ordinarily appears to surround a black patch suddenly formed upon a white ground (*loc. cit.*, p. 375) becomes transformed under strong illumination into a pale red one. White letters printed upon black paper, when seen through the disk turning the reverse way, appear to be blue under ordinary illumination, and red when the lamp is brought very near the disk.—*May* 6.]

"A Dynamical Theory of the Electric and Luminiferous Medium. Part III. Relations with Material Media." By JOSEPH LARMOR, F.R.S., Fellow of St. John's College, Cambridge. Received April 21—Read May 13, 1897.

(Abstract.)

1. It was shown by Maxwell that the theory of electric current systems flowing in rigid conductors could be formulated dynamically if the current in each circuit, which is supposed constant all round it, is taken as a generalised velocity: a formal development in his manner of the properties of the current system, from the single dynamical foundation of least action, was given in the first of the present series of papers. It is implied in such a view that the positions of the linear conductors and the intensities of the currents that are flowing in them control completely, it may be in an entirely unknown manner, the motions that are going on in the surrounding æther; just as in the cyclic irrotational motion of a perfect fluid the positions of the rigid cores round which the circulations take place, and the amounts of these circulations, determine the motion. But it was pointed out that, whereas the vorticity of such a core is dynamically a momentum, on the other hand in order to satisfy the facts the currents in the circuits must be considered dynamically as velocities; so that there is not any real analogy between the two cases.

This simple dynamical formulation is no longer available when attention is not confined to complete rigid circuits, for example when the mechanical forces acting on a portion of a circuit are in question. It is insufficient also when the electromotive force (integrated electric force) between two points of a circuit carrying a current is to be discussed, that quantity being realisable and measurable by bridging the two points through the incomplete conducting circuit of an electrometer.

A knowledge of the electric force at a point, as well as of its integrated value round a circuit, is also essential when the current in the circuit is in part conducted and in part arises from changing electric displacement: it is thus essential in the theory of electric waves and radiation, though unimportant in ordinary electrodynamic applications; and for this reason that part of electric theory remained unsettled until Hertz showed how to produce and investigate free electric waves.

Previous to that time, the theory had been completed hypothetically in various ways, by extending the range of the dynamical formulæ (with such generalisations as were admissible) from the complete

rigid circuit and the complete circuital current to the geometrical elements of which they are composed. The way was prepared for this by F. Neumann, by his discovery of the electrodynamic potential formula, from which he deduced by a uniform analytical process the mechanical and electric forces in complete rigid circuits, as previously formulated by Ampère and Faraday. Then Helmholtz took up and extended Neumann's theory, introducing Maxwell's principle that the formula of Neumann represents the kinetic energy of the system of current elements, to which Lagrange's general dynamical equations, or more precisely the action method, can be applied. His main aim was to include in this potential theory the conception of dielectric currents introduced by Faraday: but he was at the same time led to generalise Neumann's formula so as to include the possibility of waves of electric compression as well as transverse electric waves in dielectric media: the existence of the former type of waves Hertz's discoveries have disproved.

This theory of von Helmholtz thus originated by way of elucidating, from the older standpoint, the æther scheme advanced by Maxwell in the shape of a system of analytical relations, which were the mathematical formulation of Faraday's views, and involved also a unification of the electrical and optical functions of the æther. The equations of Maxwell include implicitly a definite theory of all electromotive phenomena in open circuits *at rest*, a fact which is not very obvious for the form into which they were thrown in Maxwell's "Treatise," but which is plainer for the form of two conjugate circuital relations into which he had previously cast them as being the simplest and most direct formal expression of the theory, but which he left aside in favour of a conception of electrodynamic momentum or vector potential that was intended to connect the equations directly with dynamical principles: this simple formal specification of ethereal relations has since been restated and utilised by Heaviside and Hertz. These Maxwellian fundamental equations, being purely electromotive, gave no direct explanation of ponderomotive forces: for that purpose the theory had of necessity to take on a dynamical form. A formula for the distribution of the electric energy throughout the æther was suggested by various considerations, and from it an attempt was made by the methods of general dynamics to establish the laws of the force exerted by the æther on the different parts of conductors conveying currents: and it was natural that the same procedure should be extended to an attempt to place the fundamental formal equations of the æther itself on a dynamical basis. But what was lacking for the satisfactory accomplishment of this purpose was a definite and consistent idea of how the electric charges and currents in the matter established a hold on the æther. In the absence of this, Maxwell had to rely, when actions on portions of

circuits were under consideration, on the notion of a current element, and to derive the formulæ from that conception with such degree of definiteness as was possible. It was this imperfect dynamical method which it was the aim of Helmholtz to discuss and elucidate in the analysis above mentioned.

In so far as these theories were dynamical they all involved current elements: but the criticism of the second of the present memoirs is held to show that a current element is not a legitimate dynamical entity, in that the forces strictly derivable from that assumption, both mechanical and electric, are in disagreement with experimental knowledge: and the reason is indicated, namely that the method of current elements forms an incomplete specification of the phenomena, inasmuch as it gives no account of how a current is induced within an element of volume of the matter by separation of the two electricities under the action of the electric force. This led to the introduction of the mobile electron or atomic charge of electricity as the true physical element, and to a dynamical theory of molecular type which is held to be self-consistent and in full agreement with experimental knowledge, and which may be regarded as in a manner a final development of the Weberian notion of moving electric particles. The *true* electric current of moving electrons is thus made up of a current of conduction and a current of dielectric polarisation: it does not flow in closed circuits, but if there is added on to it a quantity called the æthereal displacement current, which is not a flow of electricity at all but a flux of elastic displacement of the æther, there is obtained the *total* circuital current of Faraday and Maxwell. This more precise three-fold specification of the total current, in place of the two-fold specification of Maxwell which ignores the physical distinction between the polarisation current in the dielectric matter and the displacement current in the æther occupying the same space, introduces notable differences as regards ponderomotive forces, moving material media, electric radiation, and in other respects. In the main the results correspond with Faraday's geometrical mode of specification by means of tubes of induction; they agree more closely with the scheme derived by Maxwell from direct consideration of his mechanical model of the electric field than with the later analytical theory of his "Treatise."

The general equations of the electric field, when it contains moving material bodies, dielectric or conducting, are formulated on these principles. They are applied to the problem of the uniform rotation of a charged conductor, or a dielectric body, in a magnetic field: to the influence of motion of a material medium on the velocity of radiation that is passing across it: and to the influence of steady translation of a material system through the æther on its configuration and the distribution of its electric charges.

2. The object of the present memoir is to further develope and apply this conception of the relations of æther and matter, which has thus been shown to form a working basis for optics and electrodynamics. According to this scheme the æther is the seat of the elastic transmission of electric and magnetic force: but the ponderomotive forces acting on its electrons or strain-centres, which represent matter, are derived directly from the energy without any reference to such transmission; and this has been taken as an objection to the theory. We are therefore led to a critique of the sufficiency of the principle of step-by-step transmission or contact action as a basis for a complete connected representation of the play of physical agencies.

With a view to thus attaining more precise ideas as to what constitutes ultimate physical explanation, the properties of a material elastic solid medium involving in its constitution centres from which intrinsic strain spreads out into the surrounding parts, are noticed. The theory of fluid vortices, irrespective of any claim to form a natural representation of material phenomena, has been of fundamental service as an illustration of what kind of interconnexion it is conceivable to assume between matter and the universal æther: in vortex-atom theories the fundamental reality as regards pressure and inertia is transferred to the continuous fluid æther, and the properties of the atomic matter, which had been the original source of dynamical suggestion, reappear as secondary or derivative. Now just as a vortex atom is a state of intrinsic motion which can travel or flit through the fluid, independently of, and in addition to, motion of the fluid itself, so also can a strain-centre in an elastic solid medium move about independently of the medium.* Its motion forms an element beyond and in addition to the changing strain impressed on the parts of the solid medium by external influence. It would be easy to construct a solid body involving such strain-centres, and this circumstance gives an impression of actuality which may be felt as wanting in an abstract theory: the material of the solid would illustrate æther, the strain-form would represent an essential of matter. Such a representation is a step beyond and outside the ordinary idea of an elastic medium as the mere vehicle for transmission of forces: that notion, effective in its own sphere, would not give any account of where or how these forces originate, or what is the nature of the connexion by which the matter gets a grip upon the impalpable æther. Nor was it to be expected that the mere idea of contact action, which has come to be interpreted as an idea of elastic transmission, could of itself give account of the actions of

* The analogy is not quite complete; for the vortices move so that their cores are always made up of the same portions of the medium, while movement of a strain-centre does not to any extent carry the medium with it.

which it is only the mechanism of transmission. It is here maintained that the whole of the action between different portions of matter cannot possibly be represented as transmitted by the æther in that manner. It would even be a fair defence of this position to claim that the *onus* of demonstration lies on those who assert the opposite: but the material elastic medium, above mentioned, pervaded by intrinsic strain-centres, furnishes a crucial illustration. The parts of that medium itself are in motion on account of the changing strain; while in addition the intrinsic strain-centres put each other into motion across the medium (as vortices do across an intervening fluid) by their mutual actions. The theory of contact action so called, or elastic transmission, is not wide enough to include all this: the dynamical interactions between the strain-centres are no more transmitted by elastic action than the interactions between fluid vortices without solid cores are transmitted by fluid pressure: though in each case that agency is a necessary concomitant to the dynamical effect. The really absolute thing in dynamical explanation, that on which this principle of elastic transmission or contact action has itself been built up by Lagrange and Green, and without which it could not have assumed a precise mathematical form, is the scheme of fundamental dynamical notions connecting inertia and force, in their modern generalised aspect which formulates them under the energy principle. The main problem of transcendental physics is to assign the nature of the ultimate medium or scheme of relations which combines physical phenomena into a unity, in whose relations these dynamical notions have their scope: and it is only the prejudice of education that would keep, in this wider field, too close to the ideal of mechanical transmission in a homogeneous elastic solid.

In so far as we can collect the various dynamical principles into a simple formula, we shall attain the security that all the trains of results that come from that formula are consistent among themselves, and form parts of a single interlaced scheme. Such a formula was dimly foreshadowed by Maupertuis as the Principle of Least Action, and after various elucidations by Euler was finally established in its generality by Lagrange for ordinary material systems. The ultimate unification of physical theory, which transcends and includes ideas of contact action and other partial explanations, would thus lie in the formulation of the energy function of the æther, including matter, in a manner suitable for the application of the action analysis to the correlation of the observed phenomena. To assume that this will ever be accomplished absolutely and completely is nearly the same as to assume infinite capacity in the human understanding: but exact knowledge of inanimate nature comes by a process of analysing and classifying into types the main physical agencies as they present themselves in bulk to our senses, and it is a

legitimate and feasible problem to seek out those aspects of the underlying unity which are the cause of the interactions and correlations of these agencies, so far as they have yet been unravelled. The mere recognition of the precise order that reigns in the larger workings of the unlimited diversity that constitutes matter is itself the strongest argument that the common basis of the varieties of matter involves something simple and universal in its relations, with which we are really in physics more intimately concerned than with the infinity of arrangements and collocations that the molecules of matter can individually and collectively assume.

It follows from the analysis of the second of these memoirs that the action formula can be completely expressed and developed in a molecular theory sufficiently refined to take account of each electron separately; also that the main outline of the ordinary electrodynamic theory, for finite systems of bodies treated as continuous aggregates, can be developed from the action formula transformed so as to be expressed in terms of matter in bulk, when the currents are specified as circuital and the different regions are homogeneous as regards electric and magnetic polarisation. But in questions of details of mechanical action on matter, especially when in motion, and in questions involving the distinction between the true current carried by the matter and the total circuital current, and also involving heterogeneity in the dielectric, the pure dynamics does not suffice, and recourse must be had to direct processes of averaging, such as are necessary in other domains of molecular theory, as for example the theory of gases.

3. The utility of an elastic solid model of the kind above described is not to represent the æther, but to enlarge our ideas: for the optical phenomena show that the elasticity of the actual æther is of rotational, not distortional or elastic solid, type. It is however also explained in the memoir how, out of matter gyrostatically dominated, it is theoretically possible to construct a model which will represent the æther itself and its electrons for any assignable time, though not for ever: the properties of the model there described would gradually fade away, just as if matter were not eternal. A main difficulty in designing such a representation lies in the circumstance that it must possess the property of perfect fluidity for irrotational motions.

The distinction between the features of elastic transmission in the æther and in material elastic substances is brought out. The former medium is a pure *continuum* of which elasticity, inertia, and continuity of motion, are the sole ultimate and fundamental properties. Matter on the other hand is made up of discrete atoms or singular points in the æther; it has the inertia which belongs to these singularities individually; it has elasticity on account of their

interactions through the æther; its continuity of motion is, in the case of fluids, of limited character, being maintained only by viscosity and other such causes. The elasticity of matter in bulk is to be based on the distribution of organised material energy per unit volume: the material energy depends on the relative positions of the atoms, therefore this organised or mechanical part of it depends on the change of their mutual configurations expressed with reference to the deformation of the element of volume, that is on the strain. It follows, as Green was the first to show with logical simplicity and precision, that the stress in an element of volume is self-conjugate. But none of these ideas have any application to the æthereal *continuum*: in it there is no question of change of mutual configuration of physical parts; the energy of strain is not thus restricted to be a function of deformation only, in fact after MacCullagh it assumes the geometrically more simple form of a function of the absolute position, more precisely of the rotational displacement, of the element; the resulting stress need not be self-conjugate,—nor is it in fact so even in a material medium that has gyrostatic quality. An electric field would consist of rotational strain in the æther, a magnetic field of irrotational flow, each in actual cases extremely slight: the motion involved in a permanent magnetic field combined with a permanent electric field would not become jammed in course of time, because it can relieve itself by a slight separation of free electrifications, which will again neutralise each other after this object has been attained.

4. An atom of matter has been represented by a collocation of electrons describing stable orbits round each other. The discussion of the internal vibrations of such an atom and the consequent radiation will follow the lines of Laplace's general analysis of the oscillations about steady motion of a system of connected bodies like the Solar system. When the gyrations or orbital motions are sufficiently rapid, there will be two types of vibrations produced by disturbance of the system; very rapid ones which radiate light, and very slow ones like the precession of a spinning top which do not involve appreciable internal deformation of the system. In gases it is only these latter that would be excited sensibly by the comparatively gentle encounters between the molecules: these are in relation to the thermal energy, but are only in indirect connexion with radiation. The difficult outstanding problem of the theory of gases, that namely of the connexion between temperature and internal thermal energy, involving the relation of the two specific heats, would on this view take on a form different from the usual one. In various other respects, a recognition that the motions which constitute heat are not the vibrations which feed radiation seems to extend and improve the capabilities of molecular theory.

Another field in which the influence of a gyratory character in the molecule might be expected to be prominent is that of optics, more particularly the influence of matter as it appears in refraction and reflection. As an introduction to this subject, Lorentz's law of the relation between refraction and density is worked out; the argument is purely statical and independent of the constitution of the molecule, and closely follows a cognate investigation of Clausius as I afterwards discovered; it is however retained, as it appears to be exempt, within its proper scope, from the objections which are valid against other modes of demonstration of that law that have been proposed. When we pass on to discuss dispersion, the forced vibrations of the molecules come in: and these will be of different type according as the molecule is taken to be a system vibrating about a position of rest as has hitherto been tacitly done in optical theory, or a system vibrating about a state of steady motion as it is here required to be. The main result, for a medium devoid of non-selective opacity such as would arise from conduction, is that the Lorentz refraction equivalent $((\mu^2-1)/(\mu^2+2)\rho$ (not μ^2-1 as in the usual dispersion theories) is an additive physical constant, equal for each simple medium to $\Sigma g_r/(p_r^2-p^2)$, where p_r is a natural vibration-frequency for the molecule and g_r is a related constant. The only simplification that comes in when the gyratory quality is absent is that then g_r is necessarily positive: in the present case the reasons for taking it to be positive are not so conclusive. If in any term g_r were negative the character of the anomalous dispersion near the corresponding absorption band would be the opposite to that indicated by Kundt's law, which has hitherto always been observed to hold good.

The only way that is *à priori* unexceptionable for determining the complex index of refraction of a strongly absorbing medium such as a metal is Kundt's method of deviation by thin prisms: this gives only the real part of the index, but it is shown that by taking advantage of oblique as well as of normal incidence approximate values might be obtained for the other part as well. The fair agreement of Kundt's values with those derived from experiments on polarisation by reflection is however a confirmation that surface films are not seriously operative in the latter method, which has yielded both parts of the index. If there were no non-selective absorption, the curve representing the real part of the index would rise to infinity near each absorption band, then fall straight down to the axis, coincide with the axis for an interval, and finally again rise above it. But when there is also general absorption the curve will turn back before reaching infinity, and it will not descend as far as the axis, while the sharp corners will be eased off. These characteristics are precisely those of anomalous dispersion, for example of

Pflüger's recent dispersion curves for solid fuchsin and other selectively absorbent solid substances; but there is absolutely nothing in these general features that would not fit one theory of dispersion as well as another,—they all arise from the mere notion of sympathetic vibration. The complete values of the index along the spectrum, not merely those of its real part, must be available before any preference can in this way be established.

5. A main feature of the interaction between æther and matter consists in the bodily mechanical forcives exerted on electrically and magnetically polarised material media. In the theory a transition has here to be made between the mere aggregate or sum of the varying energies of the individual molecules of a medium, and the coordinated and averaged part of this sum which is the energy pertaining to the element of volume of the medium in bulk: this further involves the definite enunciation of a general principle in molecular mechanics which has hitherto found an application, and that a restricted one, only in the theory of capillary attractions. In a polarised medium, a distinction has thus to be drawn between the averaged individual energies from which the forces polarising the separate molecules are derived, and the organised mechanical energy of the medium as a whole, which is the aggregate of these energies after the local parts arising from the neighbouring molecules have been excluded. In this mechanical energy the bodily forces on the medium in bulk are involved: it must therefore be expressible as an analytical function of the configuration of the medium in bulk, for otherwise perpetual motions could supervene. In the study of the mechanical actions in material media, consideration of the properties of the molecules is available as a guide towards the mathematical form of the function which represents the distribution of the mechanical energy of the forces acting on the element of mass: but the province of molecular theory is ended in this general survey, and the actual values of the coefficients in the energy function must be determined by observation and experiment.

In an electrically polarised material medium an expression for the distribution of this mechanical energy is obtained, and the bodily applied forces in the material are derived from it, the tractions exerted on an interface between two media being deduced from the forces that would act on a layer of gradual transition which in the limit is taken indefinitely thin. The result is that in a medium whose molecules are polarised to intensity i' by a field of electric force F, and isotropic so that i' and F are in the same direction, there is a bodily force $(d/dx, d/dy, d/dz)\int i' d\mathrm{F}$, which could be balanced by a hydrostatic pressure $-\int i' d\mathrm{F}$, and there is also a normal traction on each interface equal to the difference in the values of $-2\pi n'^2$ towards each side, where n' is the component of i' along the normal. Thus

when a fluid medium is in equilibrium there must exist in it a hydrostatic pressure $\int i'' dF$, and in addition on each interface a traction $2\pi n'^2 + \int i' dF$ along the normal towards each side, arising from other than electric causes and balancing the electric forcive: so that to maintain mechanical equilibrium, an extraneous normal traction $2\pi n'^2$ towards each side of each interface is alone required.

6. This result differs from that of von Helmholtz's investigation, also based on the method of energy: the origin of the discrepancy is traced to the circumstance that a single continuous energy-function cannot serve for the complex medium æther *plus* matter. This difference goes to the root of things, especially in optical theory, even in cases where the resulting expressions present no difference in form. Variation of the physical constants of the medium arising from the strain involved in the virtual displacement is also included by von Helmholtz in the deduction of the mechanical forcive, thus introducing effects which are here held to be more consistently explained as physical changes arising from the molecular action of the polarisation.

Of the purely local part of the total energy of a molecular medium, there is a regular or organised portion depending on the deformation of the material in bulk, which is the energy of the mechanical stress that compensates the applied mechanical forces: the remaining, usually wholly irregular, part finds its compensation in other interactions between neighbouring molecules, which may reveal themselves in the aggregate in alterations of the local physical constants of the material as well as of its volume and other dimensions.

But in the circumstances of a medium electrically polarised this *residuum* itself involves a part which is regular in each element of volume, arising from the regularity in the orientation of the molecules which act on each other in that element. The mutual forcive thence originating may be expressed, though there is not much object in doing so, as regards the interior of an isotropic medium, as an internal molecular stress related to the lines of polarisation. When the distance between the effective poles of a molecule is small compared with that between neighbouring molecules this stress is a tension $\frac{4}{5}\pi i'^2$ along the lines of polarisation together with a pressure $\frac{2}{3}\cdot\frac{4}{5}\pi i'^2$ uniform in all directions at right angles to them: it is to be considered as balanced locally by cohesive reaction.

Under all circumstances, the forces between neighbouring molecules produce and are compensated by change of the relative configuration of these molecules; they thus produce change of the *local* physical constants of the material, and also *local* intrinsic change of volume and other dimensions, all which are proportional to the square of the polarisation; but they contribute nothing directly to the mechanical stress transmitted by the material in bulk. In a solid material, however, these intrinsic changes of configuration of

the elements of volume may not fit together consistently with the continuity of the substance, and thus secondary strains may be produced which will complicate the problem. But in fluids, in which alone experiment is feasible, no such complication can occur.

The position is aptly illustrated by the ideally simple case of a perfect gas polarised in an electric field. The mechanical forcive due to the polarisation of the gas as a whole is there compensated by change of pressure, which is transmitted. There is another regular part of the forcive which arises from actions between neighbouring molecules, so that those in the line of polarisation attract and those in lines at right angles to it repel each other, after the manner of little magnets; and this, which differs from the former by being proportional to the square of the polarisation instead of its first power, is not experienced as a mechanical force, because it is wholly compensated on the spot where it originates by slight change in the ordinarily fortuitous distribution of velocities of the molecules of the gas, by which its constitution acquires an axial character with reference to the line of polarisation so that the pressure is no longer quite the same in all directions.

7. The relation is explained which exists between this *organised* or *mechanical* energy and the *available* or *free* energy of thermodynamics. The principle of available energy, which itself is a direct consequence of the negation of perpetual motions, or rather of the negation of the unlimited availability of diffuse thermal energy, is the single essential foundation of that science. It is pointed out, that if we had no direct perception of temperature through our senses, this negation of perpetual motions would necessitate the introduction of that quantity into physics, somewhat in the same way as potential is introduced into electrical theory, and would yield a demonstration of its fundamental property. Instead then of making attempts, by the aid of special molecular hypotheses of more or less problematical character, to obtain a purely dynamical definition of temperature and an analytical demonstration of Carnot's principle, it is suggested that it is more philosophical to recognise that no physical scheme of matter and molecular action is conceivable that would involve perpetual motions (in the above sense) of matter in bulk, and to base the pure theory of thermodynamics and thermochemistry directly on this postulate.

The case of homogeneous fluid media, which acquire energy of polarisation of any kind when in a field of force, is considered in a general manner: it is shown that such media will be in mechanical equilibrium provided an extraneous traction along the normal is applied over each interface between them, of intensity equal to the difference of the densities of the mechanical energy of polarisation on the two sides of the interface.

Osmotic pressure is related to the total available energy, not merely to the mechanical part of it: the limiting or maximum value which it cannot exceed is equal to the change in available energy produced per unit volume of transpiration across the partition. An ultimate deduction of van't Hoff's law of analogy between osmotic and gaseous pressure is offered, on the foundation of the principle of available energy, which is independent of any assumption as to the character of that pressure whether purely kinetic or otherwise: if this be accepted, it will follow that no inference as to the physical state of the dissolved substance, except as regards its degree of effective dissociation, is deducible from the osmotic law. It appears to have escaped general notice that what was virtually a prediction of the law for the cases of dissolved gases is involved in the equations of von Helmholtz's discussion of the influence of dissolved gas on electromotive force, in which however the argument is based on Henry's law of solution; the law itself had indeed been formulated explicitly on similar theoretical grounds by Willard Gibbs still earlier. The influence of an electric field on osmotic pressure between dielectric fluids is estimated: this involves by cyclic processes the influence of an electric field on the vapour pressure and on the freezing point of a dielectric liquid. Some considerations connected with the nature of the process of ionisation are brought forward. The laws of chemical equilibrium, as developed by Guldberg and Waage and by van't Hoff, are placed in relation to the principle of available energy. That method is also applied in a discussion of the electromotive force of a voltaic cell, and especially of the dissipative part which is established by steady finite diffusion between solutions of different concentrations.

8. A thermodynamic application which possesses interest, both from the light it throws on the nature of magnetism and from the circumstance that in it the heat supply is calculated indirectly from the magnetic energy that runs down, is the relation between magnetic susceptibility and temperature in substances not in the very susceptible or ferromagnetic condition. According to the Weberian theory, which fits in with the present view, diamagnetic energy which is not compensated mechanically goes to the induction of Amperean currents in the molecules; while paramagnetic energy not thus compensated goes to orientating the molecules, and thus into heat. It follows that the diamagnetic coefficient is independent of temperature: on the other hand it is shown that the paramagnetic coefficient should vary inversely as the absolute temperature. These laws were discovered experimentally by Curie, who finds from a very extensive investigation that they have the same order of accuracy at sufficiently high temperatures as the ordinary gaseous laws: at lower temperatures and in ferromagnetic substances the

control of the polarised molecules arises in appreciable part from the magnetic interaction of their neighbours, thus vitiating the law as well as introducing effects of hysteresis. The well-known model of Ewing would thus represent an ideal perfect ferromagnetic in which the control arises wholly from the latter cause.

In application of the previous results as to how far physical actions can be considered as transmitted across the æther by elastic stress, the conditions are formulated under which the correlative principle utilised by Poynting is valid, that the actual rate of change with time of the organised or mechanical energy within any region is expressible explicitly as a surface-integral over its boundary.

The mechanical effects of light-waves are reconsidered in the light of this molecular theory. The conclusion is reached that such effects are wholly associated with absorption of the radiation, that no influence of perfectly transparent media on radiation can provoke a mechanical reaction. There is a mechanical force acting on an absorbing mass, in the direction of the incident radiation and equal to $\frac{E}{V}(1-m^{-2})$, where E is the energy absorbed per unit time and m is the real part of the index of refraction. Partial analogies are furnished by the mechanical effects of Hertzian radiation on a medium built up of conducting linear circuits, and of sound waves on a medium formed of a system of resonators.

As an application of the law of the mechanical forcive on dielectrics, the changes of dimensions of a condenser under electrification are considered. The problem is found to admit of exact solution if the condenser layer consists of a closed sheet, of any form, but of uniform thickness. In that case the mechanical stress in the material of the sheet proves to be simply of the type of the Faraday-Maxwell stress. The theory is compared with Quincke's experimental results: their main features are verified, including those which led Quincke to assign a wholly non-mechanical origin to the effect: but something less than half the change of volume remains over as an intrinsic electric deformation, not due to the transmitted mechanical forces.

Finally a series of practical illustrations of the mechanical theory are treated, some of which have already been employed for experimental measurement, and which are capable of still further application. The mechanical circumstances attending the refraction of uniform fields of electric force by fluid media are developed. The theory of various arrangements for measuring electric tractions and pressures in fluid dielectrics is worked out. The effect of an electric field on the velocity of ripples on the surface of a conducting or a dielectric fluid is determined: as also are the relations of electric polarisation to vapour tension and fluid equilibrium. The internal mechanical forces in a complete magnetic circuit are examined, and

also the traction between the interfaces when it is divided: and the mode of calculation of the stress in a sphere of iron in a uniform magnetic field is indicated, agreeing for this case with Kirchhoff. The mutual influence of stress and magnetisation is analysed, with reference to the experimental investigations of Bidwell.

Throughout the memoir care is taken to dispense, as far as possible, with detailed algebraic processes, which are essential for special computations and verifications, but are best evaded in the discussion of general principles. Most of the discussion is also independent of the rotational æther scheme: the great advantage of an interlacing hypothesis of that kind, which remains even when it is only provisional, is that it gives an insight into the character of the formal relations that are possible or probable between the actual physical quantities involved in it.

"On a new Method of Determining the Vapour Pressures of Solutions." By E. B. H. WADE, B.A., Scholar and Coutts-Trotter Student of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received April 26,—Read May 13, 1897.

(Abstract.)

The measurement of the vapour tension of solutions has hitherto been attended with serious errors. This is especially true of the statical methods, which have alone been capable hitherto of furnishing results at temperatures between 60° C. and 100° C.

The apparatus employed in this research was in conception similar to that described by Sakurai,* except that it was in duplicate, a divided steam supply passing through two U-tubes placed in parallel. It differed, however, from his apparatus in several important features, which cannot adequately be described in a paper of these dimensions. Suffice it to say that the pressure on the contents of the two U-tubes, being the *same*, could be adjusted to any convenient value, and that the method of thermometry being differential,† the difference only of the boiling points of pure water and solution, in their respective U-tubes was recorded. Two series of experiments were made at a pressure of 760 mm., in one of which a small external heat supply was used to compensate the condensation in the U-tubes, and a second in which this was dispensed with, and constancy in the amount of liquid operated on (which proved to be of the first importance) was

* 'Chem. Soc. Journ.,' 1892.

† Identical with that employed for many years by Mr. Griffiths.

secured by an automatic device. The latter method was much the more satisfactory and regular, and is relied upon for the numbers published.

Neither method, however, gives results differing systematically from the other. Observations were made at reduced pressures as well, and with very satisfactory results, but their publication is deferred.

The substances examined were the chloride and bromide of potassium and sodium, and chlorides of lithium, calcium, and strontium. They were for the most part obtained from Messrs. Harringtons, and their purity confirmed by analyses. As the full details both of the apparatus, observations, and calculations will be published later, the bare results are here stated.

	Boiling point, in degrees above 100°.	Concentration, in gram molecules per litre.	Ratio.
NaCl....	0·047	0·0460	1·02
	0·070	0·0685	1·03
	0·074	0·0700	1·06
	0·075	0·0720	1·04
	0·118	0·1130	1·02
	0·119	0·1140	1·04
	0·129	0·1240	1·04
KCl.....	0·054	0·0525	1·02
	0·076	0·0760	1·00
	0·084	0·0820	1·02
	0·115	0·1183	0·98
	0·183	0·1888	0·97
	0·193	0·1970	0·98
LiCl....	0·057	0·0575	0·99
	0·116	0·1140	1·02
	0·153	0·1472	1·04
NaBr....	0·046	0·0460	1·00
	0·051	0·0525	0·97
	0·095	0·1050	0·91
	0·107	0·1185	0·90
	0·160	0·1750	0·90
	0·160	0·1770	0·90
KBr	0·056	0·0529	1·06
	0·074	0·0738	1·00
	0·076	0·0746	1·02
	0·088	0·0921	0·95
	0·094	0·0984	0·95
	0·101	0·1070	0·94
	0·139	0·1500	0·93

	Boiling point, in degrees above 100°.	Concentration, in gram molecules per litre.	Ratio.
$CaCl_2$....	0·048	0·0293	1·64
	0·054	0·0325	1·66
	0·073	0·0485	1·51
	0·079	0·0510	1·55
	0·085	0·0530	1·61
	0·102	0·0645	1·58
	0·126	0·0880	1·43
	0·155	0·1080	1·44
	0·169	0·1240	1·36
$SrCl_2$....	0·035	0·0215	1·63
	0·059	0·0400	1·48
	0·083	0·0565	1·46
	0·097	0·0680	1·43
	0·122	0·0890	1·37
	0·145	0·1105	1·31

A full discussion must be reserved till a later date, but here we may notice that in all cases the numbers under the heading "Ratio" will be found to be of the same order as those calculated from the theory of Arrhenius (*e.g.*, an electrolyte of type, R′Cl, when fully dissociated, should give a ratio 1·04, and of type, R″Cl_2, 1·56). But the discrepancy always exceeds the experimental error, except in the case of potassium chloride, and is particularly great in the case of calcium chloride. The latter substance gave less defined boiling points than the others, for some unknown reason, and the experimental error is here certainly at its greatest, but not nearly great enough to account for the difference.

Certain other deductions will be made from these results and others which are accumulating, on a later occasion. My special thanks are due to Mr. Griffiths, for much invaluable assistance, and to Professor J. J. Thomson, for permission to use the Cavendish Laboratory.

"On the Passage of Heat between Metal Surfaces and Liquids in contact with them." By T. E. STANTON, M.Sc. Communicated by Professor OSBORNE REYNOLDS, F.R.S. Received April 7,--Read May 13, 1897.

(Abstract.)

The determination of the rate of transmission of heat from the surface of a heated metal to colder water in contact with it, or from hot water to a colder metal surface, is a problem of some importance

in the theory of boilers and surface condensers, but the difficulty of experimentally determining this rate and the constants involved, for cases which occur in ordinary practice, is so great that, as far as the author is aware, only approximate results have been obtained.

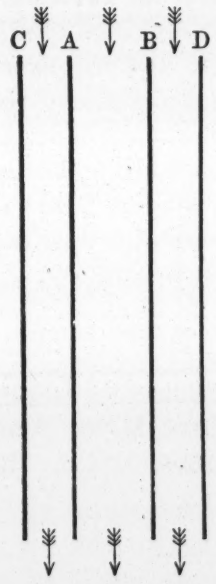
It was pointed out by Peclet that in the case of a metal transmitting heat to water in contact with it, this heat had to be transmitted by *conduction* through a film of water adhering to the surface, the thickness of the film depending on the state of agitation of the water, *i.e.*, on the convection of the heated particles of water from the surface. The determination of this rate of convection would in most cases be extremely difficult.

Another difficulty is the varying temperature of the surface of any plate giving up heat to a liquid, for in this case the problem becomes more complicated owing to the conduction of heat from one point of the plate to another.

For the experimental study of the subject the best conditions of working would, therefore, seem to be :—

1. Surface transmitting heat to water moving over it at a known velocity.
2. Temperature of the surface constant.

The first of the conditions is fulfilled by taking the case of water flowing through metal pipes; and it occurred to the author that the second would be fulfilled by the following method of working :—



AB is a thin metal pipe surrounded by a second pipe CD, the space between the pipes being used as a water jacket. If now hot water at a temperature T initially flow down the jacket, and cold water at a temperature t flow down the pipe, then heat is transmitted from the hot water to the cold water through the walls of the pipe. Again if the *quantities* in each case are the same the rise of temperature in the water flowing through the pipe is equal to the fall of temperature of the jacket water, so that at any cross-section the *mean* temperature of the water is constant.

Now the temperature of the wall of the pipe will not necessarily be a mean between the values of T and t at that point, but if the total fall of temperature from one end of the pipe to the other is small, say not more than 6° C., we may fairly assume that the ratio of the differences of temperature between (jacket-water and wall) and between (wall and water flowing through pipe) is constant for

the whole length of the pipe, and hence that the temperature of the pipe is constant throughout its length.

In addition to these conditions, the problem may be further simplified by making the velocities of the water in both pipes and jacket higher than the respective "critical" values at the given temperature, the critical value of the velocity, as determined by Professor Osborne Reynold's experiments,* being given by:—

$$v_c = \frac{1}{278} \cdot \frac{P}{D};$$

where

D = diameter of pipe in metres,

T = temperature of the water,

$P = (1 + 0{\cdot}0336T + 0{\cdot}000221T^2)^{-1}$.

In the experimental apparatus described in the paper, all these conditions were fulfilled, the apparatus consisting essentially of a pipe and jacket, as described above, two steam coils for regulating the temperature of the pipe and jacket water to any required value, and two meters for measuring the quantities of water used. The pipes used were of drawn copper, 0·08 cm. thick, so that the surface temperature could be determined from the mean temperature of the pipe, and the quantity of heat passing through the walls.

This mean temperature was determined by a delicate extensometer, which was carefully calibrated, and enabled the mean temperature of the pipe to be found within one-tenth of a degree. The initial and final temperatures of the pipe and jacket water were measured by thermometers which had been standardised.

By means of the apparatus, the following measurements could be made:—

T_0 = temperature of metal surface.

t_1 and t_2 = initial and final temperatures of the water.

p = pressure of water.

v = velocity of water through the pipe.

In this way the effect of the range of temperature, pressure, velocity, and viscosity of the water on the heat transmitted could be experimentally studied.

The results of the experiments showed that the heat transmitted from a given surface to water flowing over it,

1st, is independent of the pressure of the water;

2nd, is proportional to the range of temperature between the surface and the flowing water;

3rd, depends on a function of the velocity;

4th " " " " viscosity of the water.

* 'Phil. Trans.,' 1883, p. 976.

Or, if ds = the surface, and dH the heat transmitted, that

$$d\mathrm{H} = k \,.\, ds \,.\, v^m(\mathrm{T}_0 - t)(1+\alpha\mathrm{T}_0)(1+\beta t),$$

where m has a value little less than unity, and varies from 0·825 to 0·855 in these experiments.

It is also shown that these results are in accordance with Professor Osborne Reynold's theory of the convection of heat from a hot surface to cold water flowing over it, this theory being that the motion of heat from the surface of the pipe follows the same laws as the motion of momentum, so that from Professor Reynold's equation for the loss of pressure in a pipe,* we may write down for the slope of temperature in the pipe :—

$$\frac{dt}{dx} = k' \,.\, \frac{\mathrm{B}^n}{\mathrm{A}} \cdot \frac{q}{\mathrm{D}} \cdot \frac{\mathrm{P}^{2-n}}{(2r)^{3-n}} w^{n-2}(\mathrm{T}_0 - t) \quad \ldots\ldots\ldots\ldots \quad (1)$$

where $\mathrm{P} = (1+0{\cdot}0336\mathrm{T}+0{\cdot}000221\mathrm{T}^2)^{-1}$,

D = weight of unit volume of water,

w = velocity of water along the axis of the pipe,

r = radius of pipe,

B and A constants depending on the nature of the pipe;

and where k' will depend on the viscosity of the water at the bounding surface, through which the heat is transmitted by conductivity.

The experiments show that k' may be written :—

$$k' = k(1+\alpha\mathrm{T}_0)(1+\beta t)$$

where $\alpha = 0{\cdot}004, \qquad \beta = 0{\cdot}01$;

so that for a pipe of length L, we have from equation (1)

$$k\mathrm{L} = \frac{w^{2-n} \log \dfrac{\mathrm{T}_0 - t_1}{\mathrm{T}_0 - t_2} (2r)^{3-n}}{\mathrm{P}^{2-n}(1+\alpha\mathrm{T}_0)(1+\beta t_m)} \quad \ldots\ldots\ldots\ldots\ldots \quad (2)$$

Experiments have been made on three pipes of diameters 1·39, 1·07, and 0·736 cm., and lengths 47, 46, and 44·5 cm., respectively, at velocities varying from 28 to 394 cm. per second, and with ranges of temperature of from 30° to 3°.

The values of k are very consistent for all the experiments made, the extreme variations differing by not more than 4 per cent. for any one pipe.

The above case is for heat transmitted from the metal surface to the water. When the flow of heat is from the water to the surface,

the experiments show that the viscosity of the film of water at the surface has a much greater effect, the heat transmitted in this case being sensibly inversely proportional to the mean viscosity of the film. Experiments on the jacket water confirm this result, so that equation (2) becomes:—

$$kL = \frac{(2r)^{3-n} w^{2-n} \log \frac{t_1 - T_0}{t_2 - T_0}}{P^{2-n}} P_m \quad \ldots\ldots\ldots\ldots (3)$$

where P_m is the value of P, calculated from the mean value of $\frac{1}{2}(T_0 + t)$.

According to the experimental results, the value of the coefficient is in all cases about 5 per cent. less than the value of P_m thus calculated.

"On the Magnetisation Limit of Iron." By HENRY WILDE, F.R.S. Received April 3,—Read May 13, 1897.

In my paper "On the Influence of Temperature upon the Magnetisation of Iron," I described a new method of determining the magnetisation limit of magnetic substances, by which, with a single pole of an electro-magnet, a more exalted degree of magnetisation was indicated, as measured by the force of traction, than had previously been attained.*

The magnetisation limit of iron, as deduced from my experiments, was 381 lbs. per sq. inch of section, and it appeared to me at the time that the extreme limit was well within 400 lbs. per square inch.

I have recently had occasion to repeat these experiments with other specimens of iron of different lengths, and have increased the magnetisation limit sufficiently high to place the result on record.

Definite lengths of annealed charcoal-iron wire of circular section, 0·57 inch diameter, as measured by a micrometer screw-gauge, were cut from the same piece of wire, and mounted for suspension over the electro-magnet in the manner described in my former paper.

The specimens of iron were submitted in succession to the action of the electro-magnet excited by a current of 40 ampères, and the force of traction was measured by the weight required to detach the iron wires from the magnet, with the following results:—

* 'Roy. Soc. Proc.,' 1891, vol. 50.

Number of experiment.	Length of iron wire.	Tractive force with current = 40 ampères.	Tractive force per square inch.
	inch.	lbs.	lbs.
1	0·2	0·984	385
2	0·4	1·016	397
3	0·6	1·059	414
4	0·8	1·074	420
5	1·0	1·078	422
6	1·2	1·078	422

It will be seen from this table that the magnetisation of the first specimen agrees well with the result of my former experiment with similar charcoal-iron wire of the same length and 0·05 inch diameter, the tractive force being proportional to the sectional areas of the wires respectively. It will also be observed that the tractive force increases with the length of the wire up to 1·0 inch, beyond which no higher degree of magnetisation was obtained.

That the magnetisation limit (422 lbs. per sq. inch, or 29·67 kilos. per sq. cm.) was virtually arrived at in these experiments was shown from the fact that, when the current transmitted round the electro-magnet was reduced from 40 to 26 ampères, the amount of the tractive force remained constant.

That the single-pole method of determining the magnetisation limit of magnetic substances compares favourably with the double-pole method was proved by the following experiment:—

The electro-magnet employed in the previous experiments consisted of a single limb of the large magnet used in my demonstration of the indefinite increase of the magnetic and electric forces from quantities indefinitely small.* The second limb of this magnet was replaced, and both limbs were furnished with flat tapering poles having a space of 0·3 inch between them.

A piece of the same wire used in the previous experiments, 1·2 inch long, was bent into a U-form, with the inside of the limbs 0·3 inch apart, and carefully balanced over the tapered poles. It has been shown that the same length of wire in the straight form required a weight of 1·078 lbs. to detach it from the electro-magnet. It might, therefore, have been expected that the U-formed iron wire would have indicated at least double this amount, or 2·156 lbs., whereas the tractive force obtained was only 1·625 lbs. Some of the diminished magnetic effect was no doubt caused by the inductive action of the tapered poles upon each other, and also through the mechanical force of separation not being applied so directly as in the experiments with the straight iron wires. Enough, however,

* 'Roy. Soc. Proc.,' April, 1866. 'Phil. Trans.' 1867.

has been shown to prove that no higher degree of tractive force is to be expected from the double-pole method of determining the magnetisation limit of magnetic substances than has been obtained by the single-pole method.

May 20, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Bakerian Lecture was delivered as follows:—

BAKERIAN LECTURE.—"On the Mechanical Equivalent of Heat." By OSBORNE REYNOLDS, M.A., F.R.S., Professor of Engineering, Owens College, and W. H. MOORBY. Received March 10, 1897.

(Abstract.)

The purpose of this research differs essentially from that of any previous research on the mechanical equivalent of heat. In order to diminish the loss of heat by radiation, as well as to obtain the equivalent for water in the neighbourhood of ordinary temperatures, the ranges of temperature over which the previous dynamical measurements have been made are greatly less than the standard interval between the physically fixed points of temperature to which all thermal measures are referred, and so have of necessity involved the use of scales, the intervals of which depend on the constancy of the relative expansions of such substances as glass, mercury, and air. On the other hand, in this research the object has been to determine the mechanical equivalent of the total heat necessary to raise the temperature of water over the standard interval of temperature, and thus to obtain directly the equivalent of the mean specific heat between the freezing and boiling points.

This undertaking is the result of the occurrence of circumstances which afforded an opportunity such as might not again occur. This consisted in the facilities offered by the appliances which formed the original equipment of the Whitworth Engineering Laboratory, in 1888, the more essential of these being an engine of 100 H.P., working one of Professor Reynolds' hydraulic brakes. This brake maintains any constant moment of resistance on the engine shaft, independent of the speed, all the work done being converted into heat, which appears in the rise of temperature of a steady stream of

water flowing through the brake, the magnitude of which stream is independent of the load on the brake or the speed of the engine, and is under independent control.

The existence of the Manchester town water, of a purity expressed by 3 grains of salts to the gallon, conveniently distributed in the laboratory, as well as auxiliary power, both steam and water.

Although unconsciously, the research was really commenced in 1890, when, without any intention of making a determination of the heat equivalent of the work done on the brake, but solely for the purpose of verifying the mechanical balance of the brake, provision for thermal measurements was added, and a system of trial instituted, in which the object sought was only that of obtaining consistent results over definite portions of the scales of uncorrected thermometers, eliminating the errors resulting from radiation by taking the differences of two trials. In these trials the temperature ranged from 40° to 50° F., and their development was continued over two years. Then it occurred to Professor Reynolds that by the same method the great facility which this brake was then seen to afford would be available for the independent determination of the mechanical equivalent, if it could be arranged that water should enter the brake at the temperature of melting ice, and leave it at that of water boiling under the standard pressure. Since then all that would be required of the thermometers would be the identification of these temperatures, and with a range of 180°, small errors would be comparatively of small importance. At first the difficulties seemed formidable; but on trying by gradually diminishing the supply of water to the brake when it was absorbing 60 H.P., and finding that it ran steadily under control of its automatic gear till the temperature was within three or four degrees of boiling, he further considered the matter, and during the next two years convinced himself of the practicability of the necessary additional appliances by preliminary designs. These consisted in—

1. An artificial atmosphere or means of maintaining a steady air pressure of 4/3 atmosphere in the air passages of the brake.

2. A circulating pump and water cooler, by which the entering water, some 30 lbs. a minute, could be forced through the cooler into the brake at a temperature of 32°, having been cooled by ice from the temperature of the town main.

3. A condenser by which the water leaving the brake at 212° might be cooled down to atmospheric temperature before being discharged into the atmosphere and weighed.

4. Such alteration in the manner of supporting the brake on the shaft as would prevent excess of leakage from the bushes in consequence of the greater pressure of air in the brake, since not only would the leaks be increased but when the rise of temperature was

increased to 180° the quantity for any power would be diminished to one-sixth of that for a rise of 30°, so that any leakage would become six times the relative importance.

5. Some means by which assurance of the elimination of the radiation and conduction could be obtained, as with a temperature of 140 F. above the laboratory these would probably amount to 2 or 3 per cent. of the total heat.

6. Scales for greater facility and accuracy in weighing the water with a switch actuated by the counter.

7. A pressure gauge or barometer by which the standard for the boiling points might be readily determined at 3° or 4° F. above and below the boiling point, so as to admit of ready and frequent correction of the thermometers used for identifying the temperature of the effluent water.

8. Some means of determining the terminal differences of temperature and quantities of water in the brake, which would be relatively six times as large as with 30° rise.

These preliminary designs apparently demonstrated the practicability of the appliances, and also the possibility of their inclusion in the already much occupied space adjacent to the brake. But there remained still much to be done in the way of experimental investigation in order to obtain the data for proportioning the appliances.

In July, 1894, Mr. Moorby having undertaken to devote himself to the research, the experiments necessary for the appliances were at once commenced, and these, together with the construction of the appliances and then standardising, and preliminary experiments while this was in progress, occupied till February, 1896, when Mr. Moorby commenced the main experiments which were continued into July, 1896.

In these experiments the time of running was 62 minutes; the speed 300 revolutions a minute, on the speed gauges. Observation of speeds, of the temperature of the in-flowing and effluent water, and of the temperature of the air were made every two minutes. Observations of the slope of the temperature of the shaft were made every eight minutes.

The temperature of the inflowing water varied from 32·5° to 34°, and that of the effluent from 210° to 214° F. The effluent water was cooled to 8·5° before entering the tank on the scales, in which it was weighed, weighings being taken before and after each trial.

The temperature of the metal surface of the brake was sensibly the same as that of the effluent water (212°), and, by taking the difference in the work absorbed in two trials and the differences in the heats developed, the errors of radiation and balance in the brake were approximately eliminated; and in order to complete the elimination the coefficient of radiation was approximately determined, so that a

correction might be applied for any residual differences of temperature as observed, and in the same way with the slope of temperatures. Further assurance was also obtained by making some trials with the brake naked and others with it covered, so as to reduce the loss of heat to one-fifth; and, in the same way, with every circumstance which could affect the result trials, means were taken to vary the circumstances in different series of trials, so as to obtain an estimate of the limits of error. All the appliances were most carefully standardised; and taking all the limits of error into account the limit of the sum was less than 0·0003.

In all 52 trials were included in the final result. Of these—

25	with loads on the brake of		1200	ft.-lbs. at	70 H.P.
21	„	„	600	„	35
6	„	„	400	„	23

From these, twenty-five separate determinations were made of the equivalent, subject to certain general corrections given below, which gave a mean value 777·91. From this none of the separate determinations differed by as much as 0·2 per cent., and when arranged in eight groups, according to the circumstances under which they were made, the greatest divergence from the mean was 0·036 per cent.

It was found impracticable to eliminate entirely from each determination the losses of heat due to radiation, conduction, leakage of water, &c., and so it was found advisable to determine what these losses were. This information was given by the trials themselves, and the necessary corrections were applied to each separate determination. As illustrating the extent to which the method adopted did eliminate these errors, it is interesting to remark that on the mean value of the equivalent determined, without taking these errors into account, the error was only 0·0192 per cent.

Certain final corrections, which had the same values for all trials, were made to the mean value of J, given above. These were introduced on the following various counts:—

I. Length of the brake lever (0·00042).
II. Salts dissolved in Manchester water (0·00003).
III. Air dissolved in water used in the trials (−0·00021).
IV. Reduction of weighings to vacuo (−0·00120).
V. Varying specific heat of water (−0·00006).
VI. Pressure on thermometer bulbs (−0·00037).
VII. Work done against gravity (0·00007).
VIII. Engagement of counter (0·00001).

Their total effect was to introduce a correction factor of (1−0·00125).

The mean corrected value of the specific heat of water between freezing and boiling points, as measured in mechanical units at Manchester, is found to be 776·94.

June 3, 1897.

The Annual Meeting for the Election of Fellows was held this day.

The LORD LISTER, F.R.C.S., D.C.L.., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Dr. Gladstone and Mr. McLachlan were, with the consent of the Society, nominated Scrutators to assist the Secretaries in the examination of the balloting lists.

The votes of the Fellows present were collected, and the following Candidates were declared duly elected into the Society :—

Bell, Robert, LL.D.
Broadbent, Sir William Henry, Bart.
Chree, Charles, M.A.
Elwes, Henry John, F.L.S.
Haldane, John Scott, M.D.
Haswell, Professor William A., D.Sc.
Howes, Professor George Bond, F.L.S.
Kipping, F. Stanley, D.Sc.
Mathews, Professor George Ballard, M.A.
Murray, George Robert Milne, F.L.S.
Neville, Francis Henry, M.A.
Nicholson, Professor H. Alleyne, M.D.
Thomson, Professor John Millar, F.C.S.
Trouton, Professor Frederick Thomas, M.A.
Turner, Professor Herbert Hall, M.A.

Thanks were given to the Scrutators.

June 3, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "The Sensitiveness of the Retina to Light and Colour." By Captain W. de W. Abney, C.B., D.C.L., F.R.S.

II. "On the Mechanism by which the First Sound of the Heart is produced." By Sir Richard Quain, Bart., M.D., F.R.S.

III. "Mathematical Contributions to the Theory of Evolution. On the Relative Variation and Correlation in Civilised and Uncivilised Races." By Miss ALICE LEE, Bedford College, and KARL PEARSON, M.A., F.R.S., Professor of Mathematics and Mechanics, University College, London.

IV. "An Investigation on the Variability of the Human Skeleton with especial Reference to the Naqada Race, discovered by Professor Flinders Petrie in his Explorations in Egypt." By ERNEST WARREN, B.Sc. Communicated by Professor W. F. R. WELDON, F.R.S.

V. "The average Contribution of each several Ancestor to the total Heritage of the Offspring." By FRANCIS GALTON, D.C.L., Sc.D., F.R.S.

VI. "On the Brains of two Sub-Fossil Malagasy Lemuroids." By C. I. FORSYTH MAJOR, M.D. Communicated by HENRY WOODWARD, LL.D., F.R.S., V.P.G.S.

VII. "On the Dielectric Constants of certain Frozen Electrolytes at and above the Temperature of Liquid Air." By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, London.

VIII. "On the Dielectric Constants of Pure Ice, Glycerine, Nitrobenzol, and Ethylene Dibromide at and above the Temperature of Liquid Air." By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, &c.

IX. "On the Nature of the Contagium of Rinderpest. Preliminary Communication." By ALEXANDER EDINGTON, M.B., F.R.S.E., Director Colonial Bacteriological Institute, Cape Colony. Communicated by Sir JAMES CRICHTON BROWNE, M.D., LL.D., F.R.S.

X. "Further Note on the Influence of a Magnetic Field on Radiation Frequency." By Professor OLIVER LODGE, F.R.S., assisted by Mr. BENJAMIN DAVIS.

"On the Dielectric Constants of certain Frozen Electrolytes at and above the Temperature of Liquid Air." By J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, London. Received May 6, —Read June 3, 1897.

In a preliminary note on the "Dielectric Constant of Ice and Alcohol at very Low Temperatures"* we have described the method employed by us in determining the dielectric constants of ice and frozen ethylic alcohol at the temperature of liquid air. One difficulty which presented itself in our initial experiments was that of the construction of a suitable electrical condenser for use with frozen liquids as dielectrics. The concentric-tube pattern of condenser used by us in the first experiments on ice gave trouble by becoming deformed under the expansive force of the water on freezing, and the subsequent great contraction of the ice on cooling to the temperature of liquid air tended to break the contact between the metal plates and the dielectric. It was, in addition, difficult to clean out.

In undertaking, therefore, a more careful study of the dielectric constants of various frozen electrolytes at and above the temperature of liquid air, the first experimental problem which arose was that of the construction of a form of condenser which could be employed with the selected frozen electrolyte as dielectric, and which should comply with the following conditions:—

1. To allow any required and actual expansion of the dielectric on freezing without deforming the condenser.

2. To permit the subsequent great contraction of the dielectric substance on cooling to the temperature of liquid air, without breaking contact between the dielectric and the metal plates.

3. To be sufficiently compact to be placed in a large vacuum tube of liquid air.

4. To be rigid and have high insulation and be able to be taken apart and put together again quickly and accurately.

After trying several arrangements, we were at last able to design a condenser fulfilling the above conditions in a very perfect way, and which has given in its latest form little or no trouble in work.

This condenser, which we call a cone-condenser, is constructed as follows:—

It consists of two hollow cones turned out of hard brass (see fig. 1), the outer cone being 14·9 cm. in length inside, 5·1 cm. in diameter

* See 'Roy. Soc. Proc.,' vol. 61, p. 2, February, 1897. J. Dewar and J. A. Fleming.

inside at the top, and 2·6 cm. inside diameter at the bottom. The bottom of the outer cone is closed by a brass plate fastened on with six set screws, and which plate carries a small ebonite steady pin 1·5 cm. long and 3 mm. in diameter, screwed into the centre. In some instances a quartz pin is preferable, and can be used.

The inner cone is also closed at the bottom and open at the top. The two cones were accurately ground together and, after finishing, were thickly gilt.

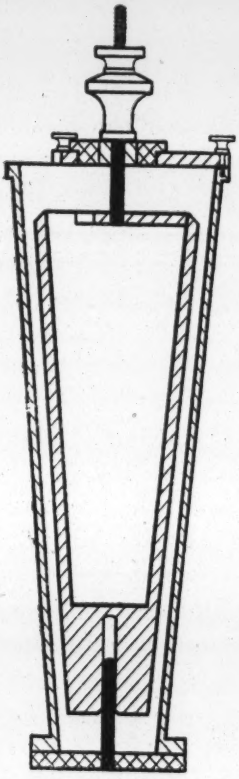


FIG. 1.—Electrical cone condenser for low temperature measurement of dielectric constant.

The inner cone is 12·7 cm. in length and 4·8 cm. in diameter on the outside at the top, and 2·6 cm. across the bottom. The bottom of the inner cone is closed by a thick plate having a recessed hole in it, into which the above-mentioned ebonite pin fits easily, and acts as a steady-pin to keep the two cones coaxial. To the top of the inner cone is fixed, by means of a three-spoke carrier, a vertical screw pin, and this screw passes through a metal boss in the centre of an ebonite bushing piece fixed in the middle of another three-spoke carrier fastened by three removable screws to the upper edge of the outer cone.

By means of a nut on the screw, the inner cone can be displaced coaxially with the outer cone, so as to leave between the cones a conical space into which any fluid can be introduced. A lock nut enables the inner cone to be fixed.

The inner and outer cone then form the two plates of the condenser, and the fluid, or frozen fluid in the conical interspace, the dielectric. The total surface of the inner cone is 147·6 sq. cm. The cross section of the ebonite steady-pin is, therefore, only about one fifteen-hundredth of the total exposed surface of the condenser, and any correction for its dielectric constant may be neglected. The weight of the whole condenser is 960 grams.

The thickness of the walls of the two brass cones is about 2 mm. in each case.

It will be seen, therefore, that if any fluid is introduced between the cones and frozen, any expansion thereby taking place lifts the inner cone, but does not in any way deform the condenser. If, then, on cooling, a subsequent contraction takes place, the weight of the inner cone makes it fall back and follow up the shrinking dielectric. And to assist this a small spiral spring is placed around the central screw between the three-spoke carriers attached to the inner

and outer cones. After the liquid is frozen, if desired, the nut working on the screw pin may be loosened, the inner cone is then free to follow up the contraction of the dielectric and always keeps up a perfect contact between the metal cones and the conical layer of dielectric. The whole arrangement is perfectly rigid, and can be taken to pieces, and put together again in exactly the same position by releasing the three screws which attach the three-spoke carrier to the outer cone.

In order to determine the temperature of the frozen dielectric a platinum wire thermometer was fixed in the interior of the inner cone with the wire pressed against the wall of the cone, and was held there by partly filling the interior of the inner cone with paraffin wax or fusible metal. Leading-in wires were then brought out to enable the resistance, and therefore the temperature of this platinum thermometer, to be instantly determined on a slide wire bridge.

The condenser was then suspended in such a way by silk strings that it could be lowered into the interior of a very large vacuum vessel, 60 cm. deep and 7 cm. wide in interior diameter, and capable of holding several litres of liquid air. The two cones of the condenser were connected with the galvanometer, battery and vibrating contact maker controlled by an electrically driven tuning-fork, just as described in a previous communication.*

The arrangement of circuits is shown in fig. 2.

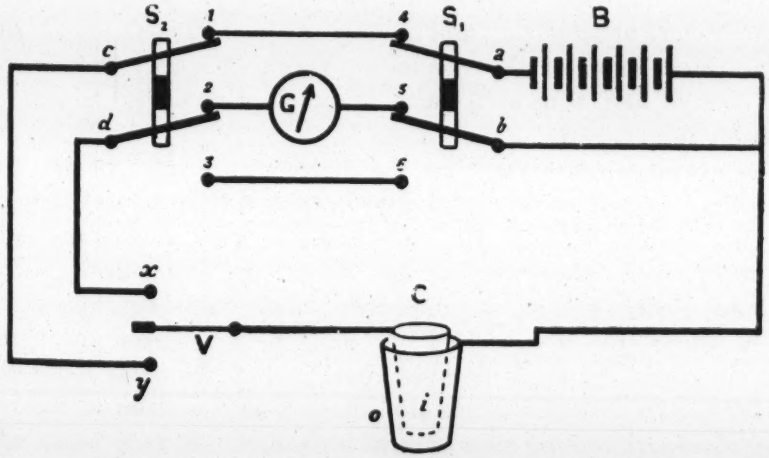


FIG. 2.—Arrangement of connections for measurement of dielectric constants. B, Battery of lithanode cells. G, Galvanometer. S_1S_2, Change-over switch. C, Cone condenser. V, Vibrating contact maker.

By means of a switch S_1S_2 the position of the galvanometer can be instantly changed from the *charge* to the *discharge* circuit, so as to

* See a "Note on the Dielectric Constant of Ice and Alcohol at very low Temperatures," 'Roy. Soc. Proc.,' vol. 61, p. 2, February, 1897.

show if the rapid series of charges of the condenser gives a current exactly equal to the rapid series of discharges. When this equality exists we know that there is no sensible leakage current passing through the condenser dielectric, or loss of charge during the interval of time which elapses between two successive contacts of the vibrating switch.

This disposition of apparatus enabled us therefore very quickly—

(1) To measure the capacity of the condenser when having any selected frozen liquid as dielectric at any temperature.
(2) To measure the resistance of the above dielectric.
(3) To determine the temperature of the dielectric layer.
(4) To heat up the whole condenser slowly to any temperature above that of liquid air.
(5) To determine whether there was any sensible leakage current through the dielectric.

The usual method of procedure, after having filled the condenser with the electrolyte to be examined, was to cool down the whole condenser to the temperature of liquid air by slowly immersing it in several litres of liquid air contained in the large vacuum tube. Then it was raised out into the cold gaseous air lying above the surface of the liquid air, in which position the whole condenser rose very slowly in temperature from −185° C. up to the melting point of the dielectric, and during this time observations for capacity, resistance, and temperature were successively and quickly taken at various temperatures.

The great mass of the condenser favoured a very slow temperature change, and thus assisted to keep all parts of the condenser at the same temperature.

The operation of measuring the dielectric constant of the frozen electrolyte placed in the condenser consisted then in measuring the current flowing through the galvanometer with a constant switch-frequency, and charging voltage, when the condenser had the selected electrolyte as its dielectric; and then again when the frozen electrolyte was melted out and the condenser put together with the cones the same distance apart and at the same temperature but having gaseous air as the dielectric. As a matter of fact, each time of experimenting one careful measurement was made of the charge and discharge current through the condenser set up with the cones a fixed distance apart and the cones cooled to the temperature of liquid air, the dielectric being gaseous air of the same temperature. The capacity of the condenser, when the vertical distance separating the cones was 1·5 mm., was very nearly 1/8000th of a microfarad, with air as dielectric. Under these circumstances the multiplication of the observed galvanometer deflection by a factor, after applying a correction for the capacity of the leads and vibrator,

suffices to reduce the galvanometer reading to the value of the dielectric constant of the dielectric used referred to that of gaseous air as unity.

The galvanometer used in these experiments was a Holden-Pitkin galvanometer with movable coil. The scale was placed 125 cm. from the mirror, and the sensibility was such that an electromotive force of 1·434 volts, acting through a megohm, gave a scale deflection of 3·6 cm.

In making the measurements a careful watch was kept to see if the charge current of the condenser was always equal to the discharge current; and the moment the charge and discharge galvanometer deflections differed by anything like a millimetre or two of scale deflection (the scale being 125 cm. from the galvanometer) the capacity measurements were no longer trusted. Moreover, at intervals, a large resistance of several thousand ohms was inserted in the galvanometer circuit to ascertain if there was any variation of deflection. The absence of any change in deflection was also taken as the proof that no sensible leakage current was passing through the dielectric, and polarisation practically absent.

From the construction of the condenser it will be seen that a considerable vertical movement of the inner cone only changes the perpendicular distance between the surfaces of the cones by about one-tenth of the vertical rise. When a liquid is placed between the cones and frozen there is a change of distance due to the lift of the inner cone. We have, however, convinced ourselves by experiment that the change in the capacity of the condenser which may occur owing to change in the bulk, either expansion or contraction, of the dielectric cannot make more than 5 per cent. change in total capacity measured. This is not more than may easily occur through unavoidable degrees of impurity in the dielectrics used. We have not thought it necessary to apply a correction for the change in dimensions of the dielectric thickness on cooling.*

In reducing the observations a correction has been made for the capacity of the connecting wires and for that of the vibrating contact maker,† and in the following tables the reduced and cor-

* There is also a correction due to the fact that some of the lines of electrostatic induction start from the inside of the inner cone and terminate on the carrier or other portions of the outer cone. In other words the whole of the capacity of the system is not comprised between the opposed surfaces of the cones. This, however, is not a large correction, as we have found that even if it is omitted we obtain results for the values of certain well-known dielectric constants closely agreeing with those of other observers. Our object in these experiments has not been so much to obtain great numerical accuracy of value as to determine the general mode of variation of the dielectric constant with temperature.

† See 'Roy. Soc. Proc.,' vol. 61, p. 12, Dewar and Fleming, "On the Dielectric Constant of Ice and Alcohol at very low Temperatures."

rected value is given of the dielectric constant of the various electrolytes used, the dielectric constant of air at ordinary pressure and temperature being taken as unity. Temperatures are all given in terms of our standard platinum thermometer P_1.*

The temperature at which the observations are interrupted in the case of each electrolyte depends upon the temperature at which sensible electric conductivity makes its appearance. One of the most remarkable facts which has attracted our attention in these experiments is that electric conductivity in electrolytes which are frozen and then reduced to very low temperatures extends far down into the solid condition, but there is for each body apparently a temperature at which electric conductivity begins to increase very rapidly, so much so, that in some cases a rise of temperature of only two or three degrees makes all the difference between being able to measure the dielectric constant well by the above-described method and not being able to measure it at all.

At the lowest temperatures reached, all the frozen electrolytes we have used appear to be practically perfect insulators, or, at any rate, to have enormously high electric resistivity, and then, as they are gradually heated up, long before their melting point is reached, quite suddenly they begin to exhibit very sensible conductivity. We have not attempted to interpret this result into the language of any theory of electrical conduction, but it is evidently a very important fact in connection with electrolytic conduction.

Hence, in the above described observations, a point of temperature is reached for each frozen electrolyte as it warms up, at which, for the frequency of charge and discharge (viz., 120 per second) we were using, further observation on the dielectric constant became impossible, at least by the method employed. Cases in which the true dielectric measurement is doubtful, owing to such incipient conduction, are indicated by a query.

The following measurements have, then, been made on certain well known electrolytes frozen as described:—

* See "Thermo-electric Powers of Metals and Alloys," Dewar and Fleming, 'Phil. Mag.,' July, 1895, p. 100.

Dielectric Constant of Sulphate of Copper.

(10 per cent. solution.)

Frequency of contact-maker 121. Galvanometer deflection when condenser has air as dielectric = 3·91 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−199·4	10·38	2·57	Charging volts 100·4.
−163·0	19·25	4·43	
−133·2	12·70	16·40	Charging volts 19·7.
−125·2	1·25	22·20	Charging volts 1·43.
−117·5	2·45	43·80	Condenser warmed up in air out
−110·7	3·45	61·50	of the vacuum vessel.
−93·3	7·90	142·0 ?	
−87·8	9·84	174·0 ?	

Dielectric Constant of Potassic Cyanide.

(5 per cent. solution.)

−197·2	11·45	2·85	Charging volts 100·4.
−168·8	1·35	24·10	Charging volts 1·43.
−162·8	1·95	34·80	Condenser warmed up in air out
−139·1	9·15	163·0 ?	of vacuum vessel.

Dielectric Constant of Perchloride of Iron.

(Saturated solution.)

−198·8	13·37	3·34	Charging volts 100·4
−162·7	14·25	3·56	
−144·9	15·65	3·91	
−133·8	3·33	4·23	Charging volts 19·8.

The above observations are set out in curves in fig. 3.

It will be seen that in each of the above three instances the dielectric constant falls to a value, near to 3, at the temperature of liquid air, but rises very rapidly in the case of the potassic cyanide solution, less rapidly in the case of the copper sulphate solution, and still less fast in the case of the ferric perchloride solution as the temperature rises.

As regards the electrical resistance, the following tables show approximately the kind of variation which takes place, but these figures are only approximations; they include the leakage of the leads and vibrating contact maker, which, though very highly insulated, are not infinite in resistance. Hence these figures merely show the

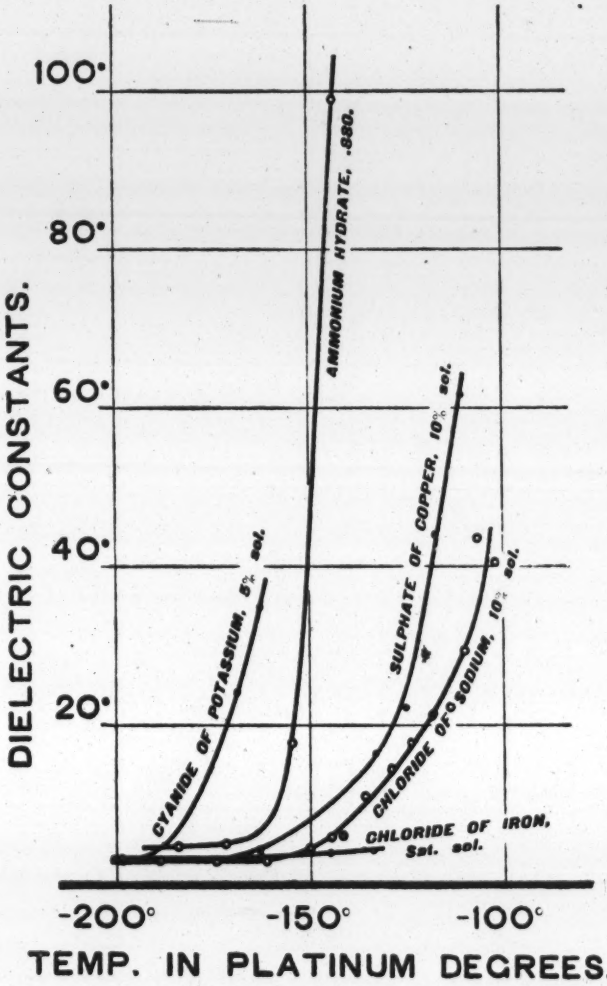


FIG. 3.—Curves showing the variation of dielectric constant of electrolytes with temperature.

probable order of magnitude of the resistance of the condenser and its mode of variation with temperature.

Resistance in Megohms of the Condenser containing various Frozen Electrolytes.

Sulphate of Copper.

(10 per cent. solution.)

Temperature in platinum degrees.	Resistance in megohms.
−200	greater than 10,000·0
−144	about 500·0
−99	about 25·0
−80	about 0·1

Potassic Cyanide.

(5 per cent. solution.)

Temperature in platinum degrees.	Resistance in megohms.
−197	greater than 2500·0
−147	about 4·0
−127	about 0·2

Perchloride of Iron.

(Saturated solution.)

−198	greater than 10,000·0
−156	about 10,000·0
−115	about 0·2

The above figures show with what great rapidity the resistance falls, when once a certain temperature is reached, which, however, is far below the melting point of the frozen electrolyte.

Further experiments were then made on chloride of sodium, hydrochloric acid, sulphuric acid, and ammonium hydrate solutions.

Dielectric Constant of Chloride of Sodium.

(10 per cent. solution.)

Frequency of contact maker 120. Galvanometer deflection when condenser has air as dielectric = 3·45 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection.	Dielectric constant.	Observations.
−198·7	2·11	2·87	Charging volts 20·2.
−197·8	10·86	2·96	Charging volts 102·2.
−188·8	10·85	3·06	
−174·2	10·88	2·94	
−161·3	10·02	2·99	
−150·2	16·35	4·52	
−144·7	21·30	5·97	
−141·7	4·39	6·15	Charging volts 20·2.
−136·0	7·85	11·10	
−129·2	10·25	14·55	
−124·2	12·55	17·85	
−118·5	14·72	21·20	
−114·0	1·10	22·30	Charging volts 1·43.
−110·2	1·45	29·30	
−106·6	2·09	43·50	
−102·0	2·00	40·40	
−92·2	3·50	65·60 ?	

At the last observation, 10,000 ohms taken out of galvanometer circuit increased the galvanometer deflection by 2 mm., showing that conductivity had begun.

Dielectric Constant of Ammonium Hydrate.

(Ordinary laboratory solution 0·88 sp. gr.)

Frequency of contact maker 120. Galvanometer deflection with air as dielectric = 2·43 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−184·2	12·35	4·82	Charging volts 101.
−171·8	12·92	5·06	
−154·8	8·70	17·80	Charging volts 20.
−143·5	3·45	98·80	

The curves for the sodium chloride 10 per cent. solution and 0·880 ammonia are given in fig. 3.

Dielectric Constant of Ammonium Hydrate.

(Dilute solution 1/100th gram molecule per litre.)

Galvanometer deflection with air as dielectric = 2·60 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−198·0	6·97	2·47	Charging volts 100·8.
−198·1	6·97	2·47	
−169·5	7·00	2·48	
−148·2	9·85	3·78	

It will be seen that the dielectric constant of the strong solution of ammonium hydrate increases much faster than that of the very dilute solution with rise in temperature.

Hence it was considered desirable to obtain an approximation to their resistances.

Ammonium Hydrate, sp. gr. = 0·880.

Electrical resistance of condenser.

Temperature in platinum degrees.	Resistance of condenser in megohms.
−180	greater than 10,000·0
−149	about 280·0
−137	about 17·0
−128	about 0·8

Ammonium Hydrate.

1/100th of gram molecule to the litre.

Electrical resistance of condenser.

Temperature in platinum degrees.	Resistance of condenser in megohms.
−198	greater than 50,000
−140	about 91

In the next place solutions of mineral acids were tried, taking sulphuric acid and hydrochloric acid as typical, and operating with them as above when frozen as the dielectric of the condenser.

Dielectric Constant of Dilute Sulphuric Acid.

(10 per cent. solution.)

Frequency of contact maker 120. Galvanometer deflection with air as dielectric = 3·45 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−202·2	2·86	4·11	Charging volts 20·1.
−197·8	2·84	4·07	
−197·5	2·84	4·07	
−190·0	2·87	4·14	
−169·8	3·00	4·21	
−158·0	4·05	5·72	
−146·8	6·30	9·00	
−142·8	8·48	12·12 ?	

Dielectric Constant of Dilute Hydrochloric Acid.

1/100th of gram molecule to the litre.

Galvanometer deflection with air as dielectric = 2·60 cm. for 100 volts.

−198·0	6·96	2·46	Charging volts 100·8.
−196·2	6·96	2·46	
−194·3	6·96	2·46	
−176·2	8·20	2·94	

The curves representing the dielectric constants of sulphuric and hydrochloric acids and dilute ammonia are given in fig. 4, which contains some other curves as well.

In the case of this dilute hydrochloric acid, the resistance of the condenser at −198° was greater than 50,000 megohms, and at

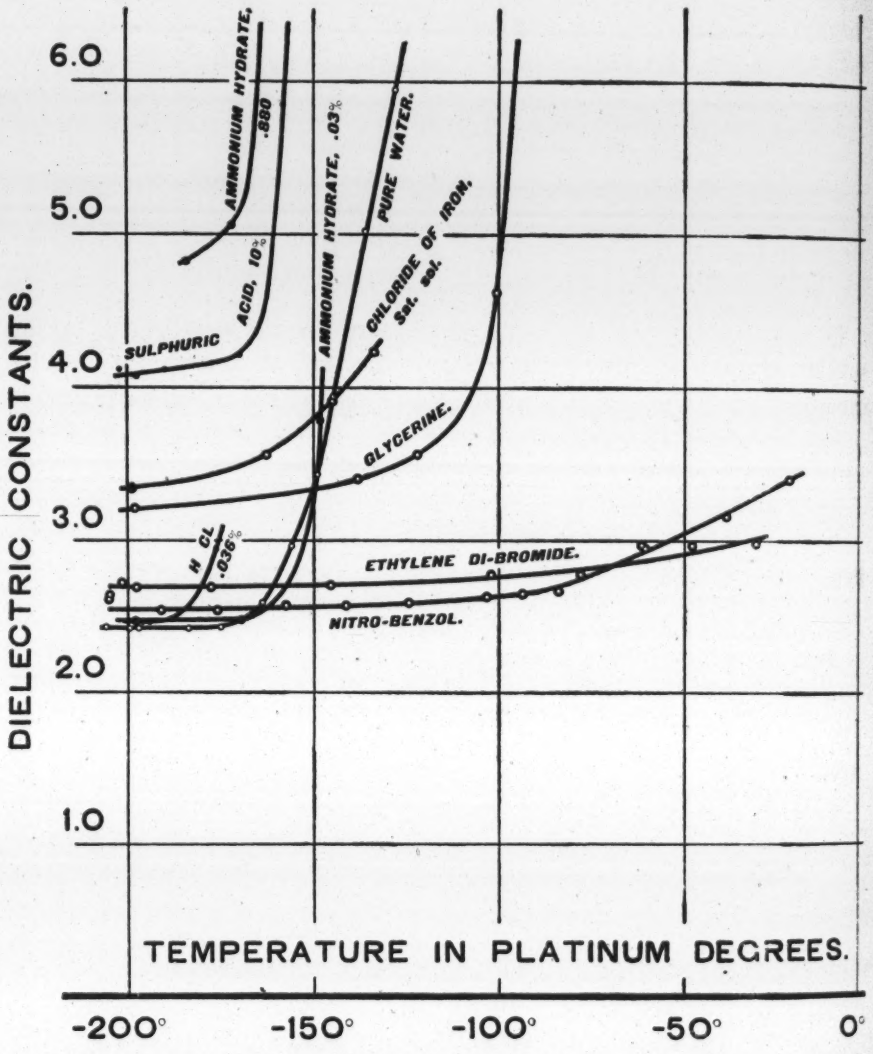


FIG. 4.—Curves showing the variation of dielectric constant of various frozen liquids with temperature.

−162° not more than 5·4 megohms. Hence there was an enormous fall between −198° and −162°.

Broadly speaking, it will be seen that in all the above cases, for the electrolytes employed, the dielectric constant of the solution, whether strong or weak, when frozen and reduced in temperature to −200°, was represented by a number not very different from that of the dielectric constant of pure ice at the same temperature; and hence it appears clear that the conductivity of the electrolytic solution at the ordinary temperature, when liquid, exercises very little influence upon the dielectric constant of the electrolyte when frozen and reduced to the temperature of liquid air. It is clear, also, that

there is a temperature for each electrolyte at which the dielectric constant and the conductivity begin to increase with great rapidity, the conductivity especially beginning with marked suddenness.

Attention was next directed to the caustic alkalis, potash and soda. Solutions of these bodies of various concentrations were employed, and the results of the observations are shown in the following table and set out in the form of curves in fig. 5.

Dielectric Constant of Potassic Hydrate.

(50 per cent. solution.)

Frequency of contact maker 120. Galvanometer deflection with air as dielectric = 2·60 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−203·2	8·23	15·6	
−149·8	12·00	22·9	
−135·6	14·57	27·8	

Solution of Potassic Hydrate.

(20 per cent. solution.)

Galvanometer deflection with air as dielectric = 3·91 cm. for 100 volts.

−201·5	2·31	41·2	Charging volts 1·434
−198·7	2·55	45·6	
−180·0	2·60	46·4	
−162·3	2·75	48·9	

In these observations 90,000 ohms were alternately placed in series with, or removed from, the condenser circuit, without producing any change in the galvanometer deflection, thus showing absence of any sensible leakage current through the condenser.

Solution of Potassic Hydrate.

(10 per cent. solution.)

Galvanometer deflection with air as dielectric = 3·36 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−203·4	3·05	63·0	Charging volts 1·434.
−202·5	3·26	67·2	
−200·8	3·65	75·2	
−197·2	4·29	87·5	
−179·0	4·34	89·7	
−170·9	4·40	90·8	
−159·6	4·43	91·5	
−150·2	4·49	92·7	
−141·1	4·60	95·0	
−129·5	4·81	99·0	
-117·4	13·2	272· ?	

Solution of Potassic Hydrate.

(5 per cent. solution.)

Galvanometer deflection with air as dielectric = 3·91 cm. for 100 volts.

−204·8	6·92	123	Charging volts 1·434.
−200·7	7·07	126	
−196·2	7·35	131	
−181·5	7·55	135	
−170·1	7·49	133	
−158·7	7·33	131	
−147·3	7·40	132	
−135·9	7·54	134	
−127·7	8·01	143	

Solution of Potassic Hydrate.

(2½ per cent. solution.)

Galvanometer deflection with air as dielectric = 2·56 cm. for 100 volts.

−201·0	10·3	20·8	Charging volts 20·0.
−166·0	14·2	29·0	
−137·2	1·6	45·5	Charging volts 1·434.

Solution of Potassic Hydrate.

(0·05 per cent. solution.)

1/100th of gram molecule per litre.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric Constant.	Observations.
−203·4	5·52	10·7	Charging volts 19·8.
−201·5	6·11	11·9	
−196·0	7·60	14·7	
−179·0	8·30	16·2	
−169·5	8·78	17·1	
−158·2	9·60	18·8	
−152·0	10·22	20·0	
−141·2	12·20	23·9	
−132·1	17·90	35·1	
−120·3	1·85	50·2 ?	Charging volts 1·43.

In the next place the same experiments were repeated, using caustic soda solution in water instead of caustic potash.

Dielectric Constant of Sodic Hydrate.

(Saturated solution.)

Galvanometer deflection with air as dielectric in the condenser = 3·15 cm. for 100 volts.

−201·8	4·55	7·02	Charging volts 20.
−181·8	4·60	7·08	
−146·7	5·00	7·70	
−102·2	6·60	10·20	
−91·8	14·75	23·10	

Solution of Sodic Hydrate.

(10 per cent. solution.)

Galvanometer deflection with air as dielectric = 3·45 cm. for 100 volts.

−199·8	2·31	50·6	Charging volts 1·434
−175·0	3·50	76·7	
−156·0	5·35	117·2	
−147·2	6·25	137·0	

The dielectric constants of these caustic potash and caustic soda solutions are given in fig. 5.

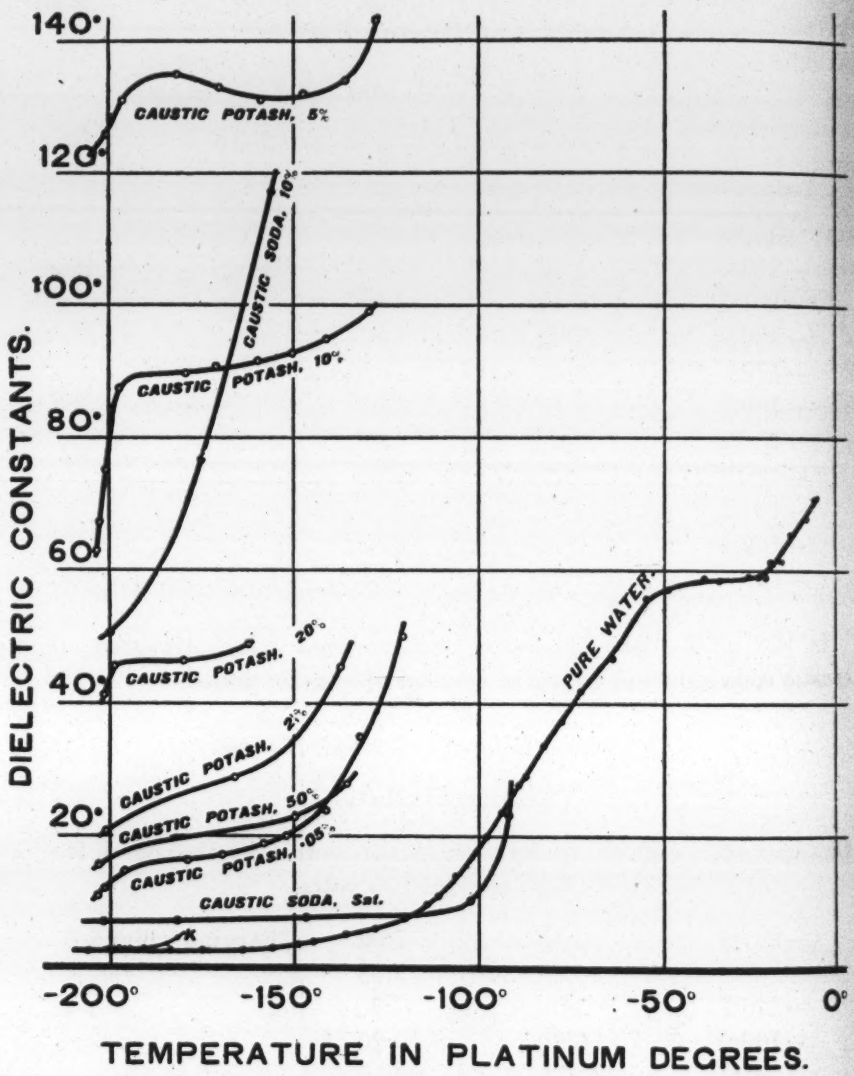


FIG. 5.—Curves showing the variation of dielectric constant of frozen caustic alkali solutions with temperature.

On examining these values for the dielectric constants of the frozen caustic alkali solutions, it will be seen that there is a remarkable difference between these last bodies and the other electrolytes examined by us, in that the values of the dielectric constant of the caustic alkalis are relatively very high at the low temperature of liquid air. In this respect the 5 per cent. solution of caustic potash is especially remarkable. For the same temperature, viz., −185° C., and varying concentrations, the 5 per cent. solution of caustic potash shows a maximum value for the dielectric constant of about 130, whereas the dielectric constant of the sulphate of copper, chloride of sodium, potassic cyanide, perchloride of iron, sulphuric and

hydrochloric acids at the temperature of liquid air all have values not far removed from 3. The dielectric constants of the caustic potash solutions thus preserve a high value until a very low temperature is reached. It is, however, clear from the diagrams that the curves representing these last constants are in each case bending downwards, and a very moderate further reduction of temperature would probably reduce the constant in these cases also to a low value.

We were desirous of seeing what effect the solvent had in the case of the caustic potash, and accordingly prepared a 5 per cent. solution of caustic potash in ethylic alcohol and examined this with the following result:—

Dielectric Constant of Alcoholic Solution of Caustic Potash.

(5 per cent. solution.)

Frequency of fork 120. Galvanometer deflection with air as dielectric of condenser = 3·44 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric Constant.	Observations.
−199·2	10·15	2·86	Charging voltage 99·8.
−200·6	10·05	2·83	1000 ohms in series with
−197·8	10·40	2·93	galvanometer.
−197·0	12·45	3·53 ?	
		cooled again	
−196·3	9·77	2·75	Charging voltage 99·8.
−194·8	1·97	2·81	Charging voltage 19·8.
−191·0	2·05	2·92	1000 ohms in series with
−184·8	2·45	3·50	galvanometer.
−181·8	3·02	4·35	

The above observations are plotted in the short curve marked K in fig. 5.

It is thus seen that the 5 per cent. alcoholic solution of caustic potash behaves quite differently to the 5 per cent. aqueous solution and has a much lower dielectric constant at −185° C. The conductivity of the alcoholic solution rises so rapidly just above −185° C. that the dielectric measurements cannot be carried up very far, but as far as they go they show that the solution in water has an immensely greater dielectric value than the solution in alcohol at −200° and upwards.

The observations we have so far made lead us to the following conclusions:—

(i) Frozen electrolytes at low temperatures are capable of acting as dielectrics even in the case of those substances which in the liquid state and in aqueous solutions have relatively a very high electrolytic conductivity.

(ii) At temperatures not far removed below their freezing points, these electrolytes have dielectric constants of large value, and numerically of the order of that of ice or water at 0° C. or in some cases much larger.

(iii) The majority of these high dielectric values so far as observed are reduced to very much lower values (near to 3) when the dielectrics are cooled to, and below the temperature of, liquid air.

(iv) Certain aqueous solutions of potash retain their high dielectric values to nearly the liquid air temperature, but give indication of being reduced at lower temperatures to small values.

(v) Frozen electrolytes at very low temperatures have very great insulating power, but recover sensible conductivity with great rapidity at temperatures far below their melting points.

(vi) It appears probable that at the absolute zero of temperature all electrolytes become perfect non-conductors of electricity or have infinite resistivity, also that it is possible their dielectric constants may all be reduced to, and represented by, a number near to 2 or 3 at that temperature, that of a vacuum being taken as unity.

We are engaged in examining as many electrolytes as possible at low temperatures to test the validity of the above last inference. In conclusion, we have again to thank Mr. J. E. Petavel for much valuable assistance in the laborious work of taking these observations, a work which has necessitated frequently very considerable endurance, in consequence of the fact that such series of observations, when once begun, had to be continued often through the night without interruption until completed.

"On the Dielectric Constants of Pure Ice, Glycerine, Nitrobenzol, and Ethylene Dibromide at and above the Temperature of Liquid Air." By J. A. Fleming, M.A., D.Sc. F.R.S., Professor of Electrical Engineering in University College, London, and James Dewar, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, &c. Received May 6,—Read June 3, 1897.

In another communication we have described the instrumental appliances we are at present using in investigations on dielectric

constants at very low temperatures,* and there is, therefore, no need to repeat these descriptions.

In the present paper we give the results we have obtained for the variation of dielectric constants at low temperatures when using certain carefully purified bodies as dielectrics.

As pure water is a standard substance the electrical constants of which have been very carefully studied, we desired in the first place to repeat our former experiments on the dielectric constant of ice, using, however, very pure distilled water as the means of procuring it.

Dr. Scott very kindly prepared for us in the Davy-Faraday laboratory a sample of pure water distilled in a silver still, and the gilt cone condenser used was carefully washed out with this water. The condenser was then kept full of this water for ten days, the water being changed twice a day. During the day-time the condenser full of water was kept at a temperature of 90° C. Having thus very carefully purified the condenser it was again filled with the pure distilled water, and this water frozen into ice by the aid of liquid air. The whole mass of the condenser and the ice being reduced to −185° C. by immersing the cones in a large vacuum vessel full of two litres of liquid air, the condenser was then raised out of the liquid air into the equally cold gaseous air lying above it in the vacuum vessel, and was allowed to warm up very slowly, taking four hours thirty minutes to rise in temperature from −185° C. to 5° C.

During this rise of temperature the dielectric constant of the ice was measured at various temperatures by the arrangements already described, and also the resistance of the ice condenser.

The corrected galvanometer scale deflection, when the condenser as used was filled with gaseous air at the ordinary temperature of 15° C., was 3·73 centimetres, using a charging voltage of 100 volts. This value is corrected for the capacity of the leads and for that of the vibrator. In the following table the temperature of the ice is given in platinum degrees in terms of our standard platinum thermometer P_1.

* See Fleming and Dewar, "On the Dielectric Constants of some Frozen Electrolytes at and above the Temperature of Liquid Air," 'Roy. Soc. Proc.,' vol. 61, p. 299, 1897; also Dewar and Fleming, "Note on the Dielectric Constant of Ice and Ethylic Alcohol at very Low Temperatures," 'Roy. Soc. Proc.,' vol. 61, p. 2, 1897.

Dielectric Constants of Pure Ice.

Frequency of contact maker 120.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant of ice.	Observations.
−206·0	9·45	2·43	Charging voltage 100·7 volts.
−199·8	9·40	2·42	
−197·2	9·39	2·42	
−183·7	9·39	2·42	
−175·0	9·46	2·43	
−164·3	10·05	2·59	
−156·0	11·42	2·96	
−149·0	13·2	3·43	
−144·7	2·97	3·94	Charging voltage 19·8 volts.
−136·0	3·76	5·02	
−128·0	4·45	5·95	
−120·0	5·50	7·38	
−114·0	7·15	9·60	
−111·0	8·08	10·8	
−106·2	10·3	13·9	
−97·8	14·65	19·8	
−93·7	17·3	23·4	
−89·4	20·5	27·6	
−87·2	1·55	29·0	Charging voltage 1·434 volts.
−82·3	1·80	33·5	
−77·7	2·0	37·3	
−72·4	2·24	41·8	
−63·8	2·57	46·8	
−54·8	2·99	55·8	
−49·0	3·05	57·2	
−39·2	3·14	58·9	
−35·2	3·13	58·5 ?	
−27·2	3·16	59·2	
−24·5	3·15	59·1	
−22·5	3·15	59·1	
−21·0	3·28	61·3	
−17·8	3·28	61·3 ?	
−15·0	3·50	65·4 ?	
−10·2	3·63	67·8 ?	
−7·5	2·78	70·8 ?	

In the case of the last four observations the discharge and charge currents of the condenser were no longer quite equal, thus showing that conductivity had begun to have influence prejudicial to the accuracy of the measurements of the dielectric constant.

The results of these observations are set out in the curve in fig. 1 marked *pure water.*

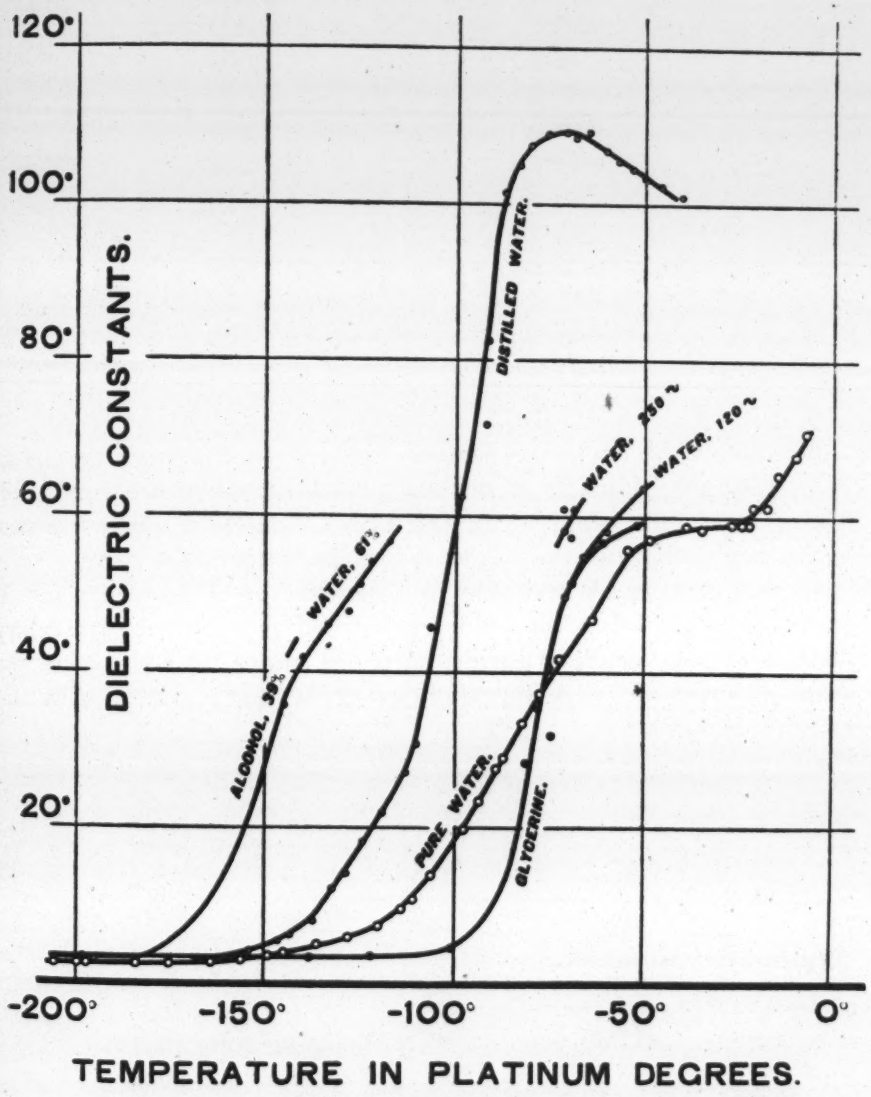


FIG. 1.—Curves showing the variation of dielectric constant of ice and frozen glycerine with temperature.

It will be seen from the form of this curve that the dielectric constant of the pure ice would be evidently represented by a number not far from 80 at the temperature of the melting point of ice.

We determined also the electrical resistance of this ice condenser during the time it was very slowly warming up to the temperature of the room, a process which occupied about six hours.

Resistance of Condenser with Pure Ice as Dielectric.

Temperature in platinum degrees.	Resistance of the condenser in megohms.
−206·0	25,000
−152·1	2,500
−138·2	2,000
−92·0	1,200
−68·2	1,200
−47·0	410
−42·1	260
−33·2	250
−27·6	40
−26·1	15
−19·2	3
−10·2	1

It is seen from the above figures that our former observations on the resistance of ice from ordinary distilled water are confirmed. Above a certain temperature there is a relatively rapid increase in the conductivity of the ice, as it rises in temperature.

It was considered desirable to ascertain the difference made in these values by using the ordinary distilled water of the laboratory instead of this carefully purified distilled water, and therefore the same experiments were repeated, using ordinary water, with the following results:—

Dielectric Constant of Ice from ordinary Distilled Water.

Frequency of contact maker 120. Corrected galvanometer deflection with condenser having air at 15° C. as dielectric = 5·63 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−205·8	17·30	2·88	Charging volts 104·0.
−205·8	3·59	2·90	Charging volts 20·2.
−199·9	17·22	2·88	Charging volts 103·7.
−195·0	17·24	2·91	
−154·0	24·7	4·16	
−146·3	6·2	5·37	Charging volts 20·3.
−143·2	7·15	6·18	
−137·2	9·15	7·97	
−132·2	13·9	12·1	
−128·6	16·2	14·1	Changing resistance in circuit from 91,500 to 1,500 ohms only altered deflection 1 mm.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−124·2	20·8	18·1	
−119·2	1·77	21·8	Charging volts 1·434.
−113·0	2·2	27·1	
−110·2	2·5	30·7	
−106·5	3·7	45·8	
−98·8	5·05	62·5	
−92·3	5·8	71·7	
−91·7	6·65	82·5	
−87·8	8·2	101·5	
−83·7	8·59	105·0	
−80·8	8·7	107·5	
−76·4	8·83	109·0	
−71·5	8·85	109·2	
−69·0	8·78	108·5	
−65·6	8·7	109·2	
−61·0	6·62	106·8	
−57·7	8·53	105·5	
−53·9	8·45	104·5	
−50·3	8·33	103·4	
−46·2	8·28	102·4	
−40·7	8·18	101·0	

Beyond this last temperature (−40·7) the observations are not of much value. Sensible conductivity had begun to make its appearance, and the measurements no longer gave the true dielectric constant.

These results are plotted in a second curve in fig. 1, marked *distilled water*. It is clear that there is in this case a well marked maximum value for the dielectric constant at about −65°, and that above that temperature the dielectric constant falls again. We have already pointed out that in the case of the ice from ordinary distilled water, a very rapid increase of electric conductivity takes place at or about −90°, and the temperature somewhat precedes the temperature at which the dielectric constant is a maximum for the ice. Other observers who have studied the variation of the dielectric constant of water at temperatures above zero centigrade, agree that beyond a certain temperature the dielectric constant decreases with rise of temperature. Thus C. B. Thwing ('Zeitschrift für Physikalische Chemie,' vol. 14, 1894, p. 296) gives the following values for the dielectric constant for water using a rapidly oscillating electromotive force :—

Temperature, Centigrade.	Dielectric constant of water (Thwing).
0°	79·46
2	80·84
4	85·20
6	80·84
7	79·40
9	77·95
12	76·20
15	75·50
20	73·92
27	72·70
40	69·80
65	64·32
80	60·50
88	57·90

L. Drude (' Wied. Ann.,' vol. 59, pp. 17—68) using electric waves 74 cm. in wave-length, determined the electric refractive index of water for various temperatures. Also Heerwagen (' Wied. Ann.,' vol. 49, 1893, p. 278) made similar determinations, using, however, a slow speed method. The results of the two last observers are given for comparison in the following table:—

Temperature, Centigrade.	Dielectric constant of water. (Drude.)	Dielectric constant of water. (Heerwagen.)
0·2°	87·3	86·96
4·1	86·02	85·55
7·9	84·49	84·17
11·6	83·41	82·83
11·9	81·20	80·90
25·8	77·99	77·70
37·9	73·44	72·67
45·4	71·28	70·60
49·8	70·01	69·00
54·8	68·60	67·21
59·7	67·17	66·02
66·0	66·02	63·14
70·1	64·83	61·66
72·8	63·68	60·66
76·3	62·82	59·41

Drude and Heerwagen do not agree with Thwing in making the dielectric constant of water a maximum at 4° C., but they all agree that an increase of temperature *decreases* the dielectric constant of water beyond that point. As we find an enormous decrease in the

constant in cooling ice from 0° C. to −185° C., it follows that there is unquestionably *some temperature at which ice-water has a maximum value for its dielectric constant.* This temperature may, and probably does, vary with the presence of slight impurities in the water, and with the frequency of the electromotive force reversals.

On examining the curves for perfectly pure water and for ordinary distilled water, it will be seen that minute impurities, as well as change of frequency, evidently have a considerable effect upon the dielectric constant at any given temperature.

This was additionally shown as follows:—

Being desirous of trying the effect of varying the frequency of the contact breaker, two sets of experiments were made with the carefully purified distilled water, the condenser being reduced in temperature to −79° C. by the aid of solid carbonic acid and alcohol.

The following values were obtained:—

Dielectric Constant of Ice from Pure Distilled Water.

Frequency of contact maker 120. Charging volts 1·43.

Temperature in platinum degrees.	Dielectric constant.
−78·0	35·3
−75·1	40·7
−71·1	49·8
−66·5	55·0
−60·2	58·2
−50·7	63·8

It will be seen that the dielectric values are all higher than those given in the former table for the corresponding temperatures.

We are disposed to attribute the differences between the values obtained in the last set of observations and the first set to the fact that the water became slightly contaminated with alcohol during the process of freezing down the condenser in the carbonic acid and alcohol. This view was confirmed by the observations taken with a mixture of alcohol and water, which showed that such a mixture rises in dielectric value as temperature increases more rapidly than does the pure distilled water.

Similar observations were taken with a double frequency of 250 and a charging voltage of 1·434.

Temperature in platinum degrees.	Dielectric constant.
−71·8	61·0
−71·5	53·2
−69·8	57·4
−69·0	61·0
−68·7	61·4

These last two sets of observations are delineated in the short curves marked *water* 250~ and *water* 120~ given in Fig. 1.

The following observations were then taken with a mixture of water and ethyl alcohol.

Dielectric Constant of Alcohol and Water.

(39 per cent. alcohol.)

Frequency of contact maker 120. Deflection of galvanometer with air as dielectric of condenser = 3·44 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−198·5	10·82	3·06	Charging voltage 100.
−145·0	25·00	36·6	
−140·2	2·07	41·8	Charging voltage 19·8.
−133·7	2·27	46·0	1000 ohms in series with
−128·2	2·35	47·7	the galvanometer.
−125·2	2·52	51·2	
−122·0	2·67	54·1	
−117·3	2·80	56·7	

The dielectric constant is seen to rise with great rapidity very soon above the temperature of −190°. This is clearly connected with the temperature at which the alcoholic mixture becomes plastic previous to fusion.

We attempted to obtain the dielectric curve of pure ethylic alcohol, to compare it with that of water, but were stopped by the fact that at a temperature very little above −200° the ethylic alcohol, although still apparently in a solid condition, becomes possessed of such an amount of conductivity that our method is no longer able to give us the true dielectric constant.

In contrast to this we have taken the dielectric constant of pure glycerine (a triatomic alcohol), with the following results:—

Dielectric Constant of Glycerine.

Frequency of contact maker 120. Galvanometer deflection with air as dielectric in the condenser = 2·43 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−198·2	8·4	3·21	Charging voltage 100·8
−138·2	8·84	3·40	
−122·0	9·27	3·56	
−100·8	11·80	4·62	

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−81·3	13·9	28·4	Charging voltage 20·0
−74·7	15·65	31·9	
−69·5	1·8	51·5	Charging voltage 1·43
−61·5	1·98	56·8	
−52·5	2·05	59·0	

The glycerine, like the other electrolytes examined, fell rapidly in resistance as the temperature rose, as shown in the following table:—

Resistance of Glycerine Condenser.

Temperature in platinum degrees.		Resistance of condenser in meghoms.
−200	greater than	25,000
−129	about	25,000
−90	,,	800
−44	,,	9

It is seen that the glycerine acts as do all the other electrolytes so far examined, its resistance falls as its dielectric constant increases.

In order to examine and compare the behaviour of water and glycerine with some non-electrolytic dielectrics, we selected for examination nitrobenzol and ethylene dibromide. Of these bodies, one, viz., mononitrobenzol, has, at the ordinary temperature and in the liquid state, a somewhat high dielectric value. The following are the results for ethylene dibromide:—

Dielectric Constant of Ethylene Dibromide.

Frequency of contact maker 120. Galvanometer deflection with air as dielectric in the condenser = 3·26 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·7	9·45	2·72	Charging voltage 100·8
−197·8	9·32	2·68	
−177·6	9·35	2·69	
−145·5	9·40	2·71	
−102·2	9·50	2·77	
−78·0	10·00	2·79	
−60·6	10·20	2·95	90,000 ohms cut out of
−48·0	10·25	2·97	galvanometer circuit,
−30·8	10·30	2·98	no change in deflection.

Ethylene dibromide, therefore, presents a remarkable contrast to water or glycerine. Its dielectric constant at ordinary temperature, 15° C., has a value of about 5.

It falls, on freezing, to nearly 3, and it continues, as seen by the above table, nearly constant down to the lowest temperature reached, at which it becomes 2·7.

Its resistance also remains, when cooled down after freezing, exceedingly high and constant, the condenser resistance being over 10,000 megohms between −200° and −30°.

From a consideration of the values which have been found for the dielectric constants of various bodies, it appears that the presence in a molecule of certain radicles such as *hydroxyl* and *nitryl* always cause high values of the dielectric constant in the liquid state. Hence we have examined one well-known nitro-substitution compound, nitrobenzol, to determine how this value is altered by very low temperatures. The dielectric constant of mononitrobenzol is given by C. B. Thwing as 32·19, presumably at 15° C. Our values for its dielectric constant at low temperatures and when solidified are as follows:—

Dielectric Constant of Nitro-benzol.

Frequency of contact maker 120. Galvanometer deflection with air as dielectric in condenser = 4·15 cm. for 100 volts.

Temperature in platinum degrees.	Galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−205·2	11·45	2·61	Charging voltage 101·0
−205·0	11·55	2·64	
−191·2	11·15	2·54	
−175·9	11·15	2·54	
−157·5	11·25	2·57	
−141·3	11·25	2·57	
−124·5	11·38	2·59	
−103·3	11·5	2·63	
−93·8	11·6	2·65	
−84·0	2·4	2·97	Charging voltage 20·0
−38·8	2·73	3·16	
−21·5	2·92	3·40	

The values of the dielectric constant of glycerine, ethylene dibromide, and nitrobenzol are plotted with some other substances as curves in fig. 2, and it will be seen that those of the ethylene dibromide and nitrobenzol values are practically constant over a wide range of temperature, and very nearly equal to that of ice at −200°.

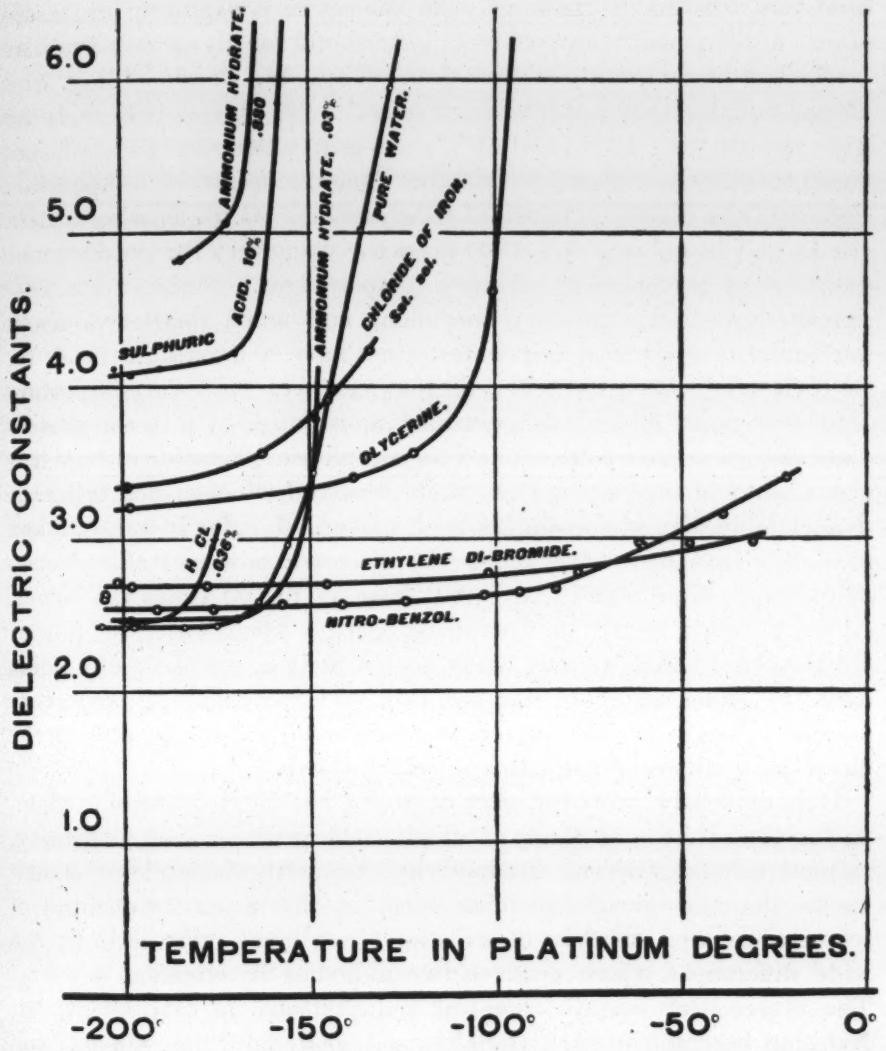


FIG. 2.—Curves showing the variation of dielectric constant of ethylene dibromide and nitro-benzol with temperature.

The dielectric constant of the mononitro-benzol value falls on solidifying at 3° C. from the high value of 32 to a low value of between 3 and 4 somewhat rapidly, and ultimately reaches 2·61 at −185° C.

In the case of these non-electrolytes, nitro-benzol and ethylene dibromide, the electrical resistance remains practically constant and exceedingly high during the rise of temperature from −200° upwards, being thus in contrast with the behaviour of the frozen electrolytes which, as shown, rise up very rapidly in conductivity at a certain temperature.

It should also be noted with regard to glycerine that other

observers who have examined the dielectric constant of this substance at temperatures above 0° C. have found for glycerine a negative coefficient of temperature variation. Thus W. Cassie ('Roy. Soc. Proc.,' vol. 46, 1889; also 'Phil. Trans.,' A, 1890, vol. 181) finds for glycerine between 18° C. and 41° C., a negative temperature coefficient equal to −0·006. Hence within this range, increase of temperature decreases the dielectric constant of glycerine. C. B. Thwing ('Zeit. für Phys. Chem.,' vol. 14, p. 293) gives the value 56·2 for the dielectric constant of glycerine at ordinary temperatures. Since at low temperatures we find a positive coefficient, and much smaller values, it is clear that glycerine, like water, must have a maximum value for its dielectric constant at a certain temperature. It is very important that this point should be examined more fully. Up to the present time electrical theory has offered no complete explanation of how it is that change of temperature has such profound effect in modifying the dielectric quality of certain kinds of matter. Ice, for instance, taken at −200° has about 2·4 times the dielectric power of the electromagnetic medium alone. At −60° from 50 to 100 times the power. At the absolute zero of temperature perhaps about twice the power. On the other hand, we may carry such a body as ethylene dibromide over the same range of temperature, without making any very sensible change in the degree to which the substance affects the electrical qualities of the space in which it exists.

It is evidently, however, very closely a chemical question. Comparing the variation of many other physical qualities, such as density, refractive index, thermal conductivity, &c., with dielectric constants, we see that the variations which occur in the latter by changes of temperature and changes of substance are much more akin to the wide differences which occur between bodies in chemical activity. The presence of certain chemical radicles, such as OH, CO, COH, NO_2, has been shown by C. B. Thwing ('Zeit. für Phys. Chem.,' vol. 14) to be a most potent factor in determining high values for the dielectric constant at ordinary temperature, but the remarkable behaviour of an aqueous 5 per cent. solution of caustic potash has shown us that questions of concentration and the nature of the solvent have to be considered as well, whilst the behaviour of all electrolytes at the very low temperature indicates that decrease in temperature destroys the influence of the radicle in this respect.

One of the most interesting points is the evidence which now exists that water, glycerine, ethyl alcohol, and no doubt many other bodies, have a maximum value for their dielectric constants at certain temperatures.

Thus W. Cassie ('Phil. Trans.,' A., 1890, vol. 181, p. 16) has shown that the curves representing the variation of the dielectric constant of glycerine, carbon disulphide, olive oil, benzol and tur-

pentine all have such a direction as to show that their dielectric constants *decrease* with rise of temperature between +10° C. and +80° C. We find that the constant for glycerine *increases* from −185° C. to −100° C., and hence it follows that it has a maximum value for a certain temperature.

This fact makes it necessary to be very cautious in drawing conclusions as to the complete form of the dielectric curve from observations made over a limited range of temperature. A somewhat hasty generalisation of this kind has been made by R. Abegg (see Wiedemann's 'Annalen,' 1897, No. 1, p. 59). He measured by Nernst's method the dielectric constants of water, alcohol, and other bodies from +10° C. down to −80° C. with such results as follows:—

Water.

Temperature.	Dielectric constant (Abegg).
+10·0°	83·8
+20·0	101·0
−40·0	112·0
−60·0	127·0
−80·0	137·0

Ethyl Alcohol.

+14·0	26·4
−33·0	33·9
−61·6	38·8
−86·6	44·3

He concludes that the dielectric constant of water would be 372 at the absolute zero of temperature, and that of alcohol would be 80 at −196°. As a matter of fact, both these constants have a value not far removed from 3·0 at the temperature of liquid air, and it is highly improbable that they increase again on further cooling.

Thus at 15° C. and at −185° C. we have the following dielectric constants for four definite compounds containing molecular groups OH and NO_2.

	Dielectric constants.	
	At 15°C.	At −185°C.
Water, H(OH)	80	2·4 to 2·9
Ethyl alcohol, $C_2H_5(OH)$	25	3·1
Glycerine, $C_2H_3(OH)_3$	56	3·2
Mononitrobenzol, $C_6H_5(NO_2)$....	32	2·6

It remains to be seen whether in all cases cooling to very low temperatures destroys the power of these radicles to bestow high dielectric values on compounds containing them when at temperatures near or above their melting points.

In conclusion, we have again to thank Mr. Petavel for valuable help in taking the above described observations, and reducing the results.

Note added June 1.

In their paper "On Capacity and Residual Charge of Dielectrics as affected by Temperature and Time,"* Dr. J. Hopkinson and Mr. Wilson give measurements of the dielectric constant of glycerine for high and low frequencies at ordinary temperatures. The value they obtain is 60 for high frequency and 50 to 60 for low frequency. They also say that glycerine has no residual charge. We obtain an almost identical value for a frequency of 120 at $-50°$, but below that temperature the dielectric curve of glycerine, we find, runs almost parallel to that of ice. It is difficult to understand why residual charge (which in ice, the above-named authors say, is considerable) should be the cause of high dielectric value at ordinary temperatures in the case of one substance and not in the case of the other.

"The Sensitiveness of the Retina to Light and Colour." By Captain W. de W. Abney, C.B., D.C.L., F.R.S. Received May 10,—Read June 3, 1897.

(Abstract.)

The author treats first of the extinction of the sensation of light on the centre of the retina. He made his reduction of the intensity of the light falling on the illuminated spot with a new piece of apparatus, which consisted of a gelatine wedge bent so as to make an annulus. He describes this wedge and its graduation, showing how its readings can be utilised, they being proportional to the logarithm of the intensity of light passing through it.

It is found that the smaller the spot of illuminated surface the less reduction in intensity of the light is required, and that the amount of reduction of the light falling on the spot which just produces no sensation of light is connected with the size of the spot by a simple formula, $I = x^m$, where I is the intensity and x the diameter of the spot. Further, he finds that it is the smallest diameter which governs the necessary reduction in intensity and not the area of the

* 'Phil. Trans.,' A, vol. 189, 1897, p. 134.

illuminated surface. Having experimented with the extinction of light at other parts of the retina, he finds that it obeys the same law. Since a large and a small area having the same actual illumination appear to be of different brightness, an investigation was made of the relative luminosities of the two, and it was found that the two were connected by a very simple law.

The reduction of the intensity of a coloured ray to extinguish all colour was next measured with areas of different dimensions, and it was shown that again the intensity of the reduced light was connected with the size of the spot by a simple expression similar to that of the extinction of all light, but the exponential coefficient differed, indicating that light and colour were not connected together in the manner which might be expected.

The author then deals with the question of colour fields, and finds that all colour fields are of the same form, the extent depending solely on the illumination and the area of the surface the image of which falls on the retina. He finds that there is a connection between the intensity of the colour and the extent of the field which can be expressed by a formula, as also can the connection between the size of the spot of illuminated surface and the extent of field. He gives the curves of illumination for equal colour fields, and the curves of extent of field for every colour in the prismatic spectrum. Finally he makes an investigation into the relative sensitiveness to light of various points in the retina, and shows that there are "iso-lumes" or fields of equal sensitiveness which appear to be of the same form as the colour fields.

He points out that there are difficulties in reconciling these results with either the Young or Hering theory of colour vision, and suggests a modification in the accepted theory of light and colour which may explain the connection between the two.

"On the Mechanism by which the First Sound of the Heart is Produced." By Sir RICHARD QUAIN, Bart., M.D., F.R.S. Received April 29,—Read June 3, 1897.

It is a well-recognised fact that the action of the heart is accompanied by the emission of certain sounds, which are described as the first and second sounds of the heart. These sounds, which were observed soon after Laennec had discovered the use of the stethoscope, have been compared to the sounds produced by the utterance of the words lŭbb-dŭp. They have been studied with interest by the physicist, the biologist, the pathologist, and the clinical physician, by the latter especially, inasmuch as the changes produced by disease in the character of these sounds become of material assistance in the

diagnosis and treatment of diseases of the heart. An explanation of the mechanism by which phenomena so distinct, so constant, and so remarkable are produced, has been sought for by many observers amongst the classes just mentioned.

The *second sound* is the result of the sudden tension of the semilunar valves, caused by the resistance which these valves offer to the retrograde flow of the blood from the aorta and pulmonary artery respectively into the ventricles on the cessation of systole. The sound is similar to that produced when a piece of tape or ribbon is suddenly made tense. It is needless to say more here on the subject of the second sound and its causes, for the explanation just offered is, I believe, almost universally accepted.

The cause of the *first sound* is, on the contrary, still a subject much discussed and undecided. Many explanations have been offered at various times of this phenomenon. Professor Michael Foster, to whose admirable exposition of the mechanism of the circulation I am deeply indebted for guidance in working out the subject of this communication, observes that "this sound presents many difficulties in the way of a complete explanation."* The difficulties result from the number of events which occur simultaneously with the systole of the ventricle and the occurrence of the sound. It seemed to me to be desirable, amidst such differences of opinion, to solve if possible a problem which has its special interest and its special importance. Two of the most striking events which take place during systole, namely, the closure of the auriculo-ventricular valves and the muscular contraction of the ventricular walls, are regarded by many authorities as the sources from whence the first sound proceeds. The result of my investigations, on the one hand, leads me to the conclusion that neither of these explanations is satisfactory; and, on the other hand, enables me to indicate what I believe to be the real explanation of the phenomenon.†

A. The action of the auriculo-ventricular valves is not the source of the first sound of the heart.

The mechanism of these valves (the mitral and tricuspid) and their action do not possess the elements necessary for the production of such a sound. The action of the valves commences when the blood, pressed from behind by the contraction of the auricles, flows into the ventricles. It is there reflected from the walls of these cavities, and presses on the lamellæ of the valves, which are thus

* 'Text-book of Physiology,' 6th Ed. (London, 1893), p. 239.

† I have to acknowledge with thanks the assistance rendered me in arranging my notes of this memoir by my valued friend, Dr. Mitchell Bruce, during my recent severe indisposition.

brought face to face into close apposition, and are so far closed by the mere pressure of the blood from behind. As soon as the ventricles are filled with blood the systole commences. The apex of the heart approaches the base. At the same time the musculi papillares contract, and by means of the chordæ tendineæ, attached to the margin of the valves, prevent the laminæ of the valves and the blood from being pressed backwards into the auricles. In this action—the simple apposition of the laminæ of the valves, and the closure of the orifices by means of the musculi papillares and chordæ tendineæ—there is no such tensive force exercised as would be sufficient to produce the loud and characteristic first sound of the heart.

Further evidence on this point may be found in another direction—namely, in the fact that the first sound can be heard independently of the existence and action of mitral and tricuspid valves. Aware of the fact that in some of the lower animals, more especially in the reptile class, the auriculo-ventricular valves exist in but a very rudimentary form, I obtained permission to examine some of these creatures in the Zoological Gardens, and spent many hours on many occasions in the investigation, assisted by my valued friend Dr. John Sibbald, who was at that time my clinical assistant at the Brompton Hospital, and who is now Senior Commissioner in Lunacy in Scotland. Having examined many animals, we finally decided that the python (*Python solurus*) afforded the best illustration of the occurrence of the first and second sounds of the heart. In the heart of that animal, of which I present two drawings, copied by permission from a preparation in the Hunterian Museum, it will be seen that the structures which represent the auriculo-ventricular valves are formed by a continuation of the septum of the auricles; they are merely muscular flaps, destitute alike of musculi papillares and chordæ tendineæ, and are without means of producing valvular tension. I may here add that in an animal of a totally different class, namely, the kangaroo, the sounds were distinctly recognised, though the valves, as seen in the second drawing (copied also by permission from a specimen in the Hunterian Museum), are very rudimentary in character.

Objections will no doubt suggest themselves to the view above expressed. For example, the clinical physician will say, "I am in the daily habit of hearing a distinct murmur accompanying the systole in cases of diseased heart, and apparently replacing the first sound. On examining the heart after death I find the auriculo-ventricular valves diseased, a condition with which I therefore connect the murmur." It will at once be seen that we have here to deal with morbid sounds totally different in character, and totally different in the seat and mode of production, from the first sound of the

heart. To make the explanation more simple, I will confine my observations to two principal forms of mitral disease associated with systolic murmur, namely, first, imperfection in structure with distinct regurgitation, and, secondly, induration, roughness, and irregularities on the mitral valve.

(*a*) In mitral regurgitation consequent on disease affecting the margins of the valves, the auriculo-ventricular orifice remains imperfectly closed; and when systole of the ventricle takes place, a portion of the blood is driven backwards towards the auricle, the remaining portion being sent forwards in its normal course towards the orifice of the aorta. The murmur which is produced by the retrograde flow of blood, permitted by the diseased valve, is heard at the apex of the heart; but at the same time the healthy first sound may almost always be recognised at the base of the heart, over the aortic valves.

(*b*) In a second form of disease of the mitral valve, there may be roughness, induration, or other irregularities from the presence of deposits on the external surface of the laminæ which meet the current of blood going towards the aorta and flowing parallel with the surface of the valves. A murmur is there produced audible at the apex, but also accompanying the current of blood towards the base, where it can be heard, often masking by its loudness the first sound. It is quite remarkable how slight the roughness or irregularity may be on the laminæ of the valves which produces a loud and definite murmur. These murmurs, striking and characteristic as they are, are merely accidental complications which occur at the moment of the systole of the heart; but they are unconnected with, and have no relation except in point of time to, the healthy first sound, which may be heard apart from, and independently of, them.

The weight of evidence, then, is clearly against the possibility of the structure or the functions of the auriculo-ventricular valves being the source from whence proceeds the first sound of the heart.

Two other phenomena occur synchronously with the systole of the heart, and consequently with the occurrence of the first sound. They are—(*a*) *the contraction of the muscular walls* of the ventricles, and (*b*) *the propulsion and movement of the blood* from the ventricles into the arteries. I shall first consider the supposed share which muscular contraction has in the formation of the sound.

B. The muscular contraction of the walls of the heart during systole is not the source of the first sound of the heart.

The sound produced by muscle during its contraction was first described by Dr. Wollaston.* He compared it to "a sound which

* 'Philosophical Transactions,' 1810.

resembles most nearly that of carriages at a very great distance passing rapidly over a rough pavement."* It is very difficult to conceive the slight, soft, rolling sound produced by muscle in action being convertible into the loud, booming first sound of the heart. Yet the theory is accepted. If muscle during contraction could produce so marked a sound, we should expect to find that the powerful muscles of the neck attached to the base of the skull and those attached to the jaw (being through the bones of the skull in direct relation with the hearing apparatus) would give us some striking evidence of the production of muscular sounds when they are thrown into strong action. It is reasonable, too, to believe that such muscular sounds must occur and be heard, if they exist, during the movements of the athlete or the boxer as well as during the performances of the *danseuse*. But there is nothing of the kind. I have failed to hear such sounds when listening to the powerful contraction of the biceps, or on listening to the contraction of the shoulder muscles of a strong cart-horse struggling with a heavy load in ascending a hill. I could hear no other sound save the soft, rolling sound described by Dr. Wollaston. Still many observers have argued that the contraction of the walls of the heart differs from the action of the skeletal muscles, and that it is this peculiar form of contraction which causes the first sound. They have adduced so many observations in favour of this doctrine that it will be necessary to examine them. But before doing so, for the sake of making my argument more clear, I desire to point out that there is another event which occurs simultaneously with the systole of the ventricle. This is the propulsion of the blood from the ventricles and its impact against the column of blood resting on the semi-lunar valves.

With a view to showing how large a share the sound of muscular contraction has in producing the first sound, observers have cut off altogether the supply of blood from the cavities, and on listening during the contraction of the heart have heard a systolic sound. Such were the old experiments of Ludwig and Dogiel, represented as confirmed by Krehl† and by Kasem-Beck.‡

The conclusions which have been drawn from these experiments are disproved by those conducted by my friend Professor Halford, and described in his essay on "The Action and Sounds of the Heart," published by Churchill (1860). He writes: "Large dogs

* It is interesting to note that Dr. Wollaston, in examining the sound produced by the muscles of his leg, made use of a wooden rod to convey the sound to his ear. He may be thus said, in a measure, to have anticipated the principle of mediate auscultation discovered by Laennec in 1816. The muscular sound which we now recognise is that accurately described by Wollaston.

† 'Du Bois-Reymond's Archiv,' 1889, p. 253.

‡ 'Pflüger's Archiv,' vol. 47, p. 53.

were obtained, and, as in my preceding experiments, the heart was exposed, and the circulation kept up by artificial respiration. A stethoscope being applied to the organ, sounds were distinctly heard. The superior and inferior venæ cavæ were now compressed with a bull-dog forceps, and the pulmonary veins by the finger and thumb; the heart continuing its action, a stethoscope was again applied, and neither first nor second sound was heard. After a short space of time, the veins were allowed to pour their contents into both sides of the heart, and both sounds were instantly reproduced. On the veins being again pressed, all sound was extinguished, notwithstanding that the heart contracted vigorously. Blood was again let in, and both sounds restored. I have thus frequently interrogated the same heart for upwards of an hour, and always with the like result" (p. 25).

These experiments of Professor Halford must be accepted as sufficient to refute the view of the German observers just quoted. Another source of fallacy in making experiments of this kind is that a very slight stroke of the muscle or ventricle against the end of the stethoscope is sufficient to produce a very loud and distinct sound, resembling the first sound. If the palm of the hand, for instance, be pressed gently over the ear, and the back of the hand be touched by a finger of the opposite hand, it will be felt how slight a stroke is sufficient to produce a very distinct sound. Observers (Ludwig and Dogiel) have recognised the difficulty of isolating muscle from the instrument conveying the sound, and they have placed the empty heart, still contracting, in a jar containing defibrinised blood or warm water, from the side of which a neck is projected, covered by a thin layer of india-rubber. At each contraction of the heart a distinct sound, resembling the first sound of the heart, was heard. But the flapping of the heart against the water was quite sufficient to produce the sound which was conveyed to the ear; and I cannot see in such an experiment sufficient evidence of the sound of muscular contraction being the cause of the first sound of the heart.

Experiments of another kind have been employed to show that the contraction of the muscle is a source of the sound. Hürthle* and Einthoven† show graphically that the first sound begins with the very beginning of the systole, before the ventricle has got power "to open the valves." This observation is entirely consistent with the view I am about to propose. The moment the ventricle begins to contract, the impact of the blood against the semi-lunar valves commences, producing the commencement of the sound, not when the valves are thrown open. The valves being connected with the fibroid

* 'Pfluger's Archiv,' vol. 60, p. 263.

† *Ibid.*, vol. 57, p. 617.

ring surrounding the base of the heart, with which also the muscular walls are continuous, the sounds are conducted to the apex.

Pathology confirms this view. It was observed by Dr. Stokes that in the course of typhus fever the first sound of the heart gradually disappears. After death it is found that the walls of the heart are softened; and this morbid state has been accepted as a proof that muscular contraction is the cause of the first sound. The real explanation is that the impulse of the heart is so feeble that it is unable to produce the sound at the valves. The correctness of this view is confirmed by the fact, recorded by Dr. Stokes, that the last point at which the sound disappears is over these valves, and that it is at the same point that the returning sound is first heard.

With the object in view of further investigating this subject, I requested my friend, Dr. Alexander Morison, to study the heart of a turtle recently killed by holding it close to the ear as one holds a watch when testing for deafness, and also by pressing it gently against the ear. With this intention, I gave him an introduction to the proprietors of the "Ship and Turtle" tavern, where he had the fullest opportunity of carrying out the investigation.

Dr. Morison writes to me:—"An opportunity was afforded me of examining the hearts of two large turtles immediately after they were killed. The heart removed from the body was easily provoked into active contraction by a gentle tap with the finger, the contraction being sufficient to expel blood from the cardiac cavity. On placing the ear close to the heart whilst thus contracting, no sound could be heard; on placing the ear lightly on the heart whilst contracting, no sound could be heard. So far, therefore, as a turtle's heart is concerned—no other that I know of has more vigour of contraction after removal from the body—muscular contraction, as a cause of cardiac sound, must be excluded."

These facts and observations are, in my opinion, sufficient to prove that the contractile action of the muscles of the heart is not capable of producing the first sound of the heart.

Having thus endeavoured to show that the cause of the first sound is independent alike of auriculo-ventricular action and of muscular contraction of the walls of the heart, I proceed to consider a third event, the most striking and important of all those which occur during the systole of the heart, that is, the propulsion of the blood contained in the ventricles into the pulmonary artery and the aorta, and herein to indicate what seems to me to be the agency by which the sound is produced.

C. The first sound of the heart is caused by the impact of the blood driven by the action of the muscular walls of the ventricles against the block produced by the columns of blood in the pulmonary artery and aorta which press upon the semi-lunar valves.

I would adopt the explanation suggested by my valued friend, the late Dr. C. J. B. Williams, that "sound is a phenomenon resulting from resisted motion." We hear it in the whistling of the wind in the rigging of a ship; we hear it when the waves break upon the shore; we hear it when the blacksmith strikes the anvil with his sledgehammer; we hear it in the gentle sounds of the Æolian harp, and in the whispers emitted by the vocal chords when the air passes over them from the trachea. Let us examine the condition of the circulation with regard to this particular point, namely, resisted motion.

In this inquiry I have derived most important assistance from the admirable and elaborate researches of Professor Bell Pettigrew, F.R.S., first, on "The Arrangement of the Muscular Fibres in the Ventricles of the Vertebrate Heart," published in the 'Philosophical Transactions' of the Royal Society, 1864, and, secondly, on "The Relations, Structure, and Functions of the Valves of the Vascular System," published in the 'Transactions' of the Royal Society of Edinburgh, 1864. These refer especially to the left ventricle, but, at the same time, it is to be observed that what applies to this ventricle applies, only in a lesser degree, to the right ventricle. Dr. Pettigrew explains the manner in which the column of blood, projected from the heart into the aorta, is formed by the union of three columns, an arrangement which results from the mechanism of the heart as fully described by him. These columns ultimately unite into one before reaching the orifice of the aorta. The columns have a spiral motion, which is the result of the spiral arrangement of the *musculi papillares*, of the spiral arrangement of the fibres composing the walls of the ventricle, as well as of the spiral shape of the left ventricular cavity itself. These points, illustrating the character of the flow of the current, are shown in the blood-cast from the interior of the left ventricle of a horse which, by permission of the President of the Royal College of Surgeons, I am able to submit to your examination.

By this spiral, or what might be called "rifle," motion, the blood is seen to be directed against the segments of the semi-lunar valves, which are thereby hastily thrown apart, the spiral current being continued for some distance within the aorta. The beautiful rifle mechanism here described is constructed with the definite objects of giving precision to the direction of the moving body against a given point, and of securing greater velocity and force in that body—the moving column of blood. We have, in fact, here represented in

nature—a matter of the deepest interest to the biologist—the mechanism of the comparatively modern rifle.

The resistance to the stream of blood issuing from the ventricle is offered by the block formed by the column of blood resting on the aortic valves. These in their action are described by Dr. Pettigrew as "closed by a spiral movement, by which these valves are wedged, and, as it were, screwed, more and more tightly into each other"; the movement here—the spiral movement—being caused by "the direction of the sinuses of Valsalva, which curve towards each other and direct the blood in spiral waves upon the mesial line of each segment." *

We find that various estimates have been given of the absolute propelling power of the ventricles and of the resistance of the column in the pulmonary artery and in the aorta respectively. For example, Professor Michael Foster says:—"If we take 180 grams as the quantity in man ejected at each stroke at a pressure of 3·21 metres of blood, this means that the left ventricle is capable at its systole of lifting 180 grams 3·21 metres high, *i.e.*, it does 578 gram-metres of work at each beat." †

Different estimates of the propelling force are given by physiologists, and the estimates of the resistance vary more even than the estimates of the propelling power. It will therefore suffice to say that authorities are substantially agreed that the driving power and the resistance are in the proportion of 4 to 3, the really important point for our present purpose being the relation they bear to each other.

In the motion thus described and the resistance we have all the elements for the production of a sound; and a sound being produced, we ask what it is. The reply must be: the first sound of the heart, the cause of which we now seek.

If it be admitted that sound is a result of resisted motion, we have in this instance a remarkable illustration of movement and resistance. The movement of the blood with all the force, precision, and velocity of a rifle or spiral movement, is directed against a fixed and definite resistance, the moving power and the resistance being capable of definite measurement and found to be quite sufficient themselves to explain the source of the sound of which we are in search. This

* 'Transactions of the Royal Society of Edinburgh,' vol. 23.

† As an illustration of the great mechanical force exercised by the muscular walls of the heart, Professor Michael Foster says that "the work of the whole heart during the day would amount to 75,000 kilogram metres, which is just about the amount of work done in the ascent of Snowdon by a tolerably healthy heavy man" ('Text-book of Physiology,' by M. Foster, M.A., M.D., London, 1893).

Professor Haughton, F.R.S., in his learned and most interesting work on 'The Principles of Animal Mechanics,' says that "the daily work of the left ventricle is equivalent to lifting 89·7 tons through 1 foot" (Longmans, London, 1893).

explanation was first suggested to my mind many years ago by the occurrence of a case of disease in which the aortic valves, being completely broken down by disease, became inadequate to their function. A murmur of such intensity was produced that it was audible at 2 or 3 inches from the wall of the chest without any intermediate communicating body. With a view to determine how far this murmur might be conducted along the course of the circulation, I examined the femoral artery. There I found that the murmur was not conveyed to the ear, but a sound precisely resembling the first sound was heard, a sound caused by the pressure of the stethoscope resisting the motion of the blood in the artery, which, the valves being destroyed, thus received the full force of the ventricular contraction. My attention was thereby directed to the obstruction offered by the aortic valves in health to the blood in leaving the ventricle. I felt the case to be so interesting that I submitted it to a meeting of the Harveian Society; and my observation is recorded in the Minutes of the meeting April, 1852.

From that time to the present I have taken every opportunity of investigating the subject. Observations have been made on the course of the circulation in the femoral artery under like circumstances by Duroziez, Moulie, von Bamberger, and others. These observations, however, have had reference more especially to the diagnosis of disease of the aortic valves, and I mention them here only as indicative of interesting investigations having been made on the femoral artery in cases of diseased aortic valves. The observers named make no special reference to the first sound of the heart in health.

Objections may be offered to the explanation of the first sound of the heart here brought forward, founded on the fact that this sound is heard more distinctly at the apex of the organ, a point removed from the seat of the valves. The observation is perfectly correct, but the explanation is simple. The muscular walls of the heart are connected, as above described, with the fibroid ring which is intimately associated with the semi-lunar valves. The sound produced at these valves is communicated to the apex of the heart through the fibroid ring and the muscular walls, which at the moment of systole are tense and firm. The sound thus conducted reaches that portion of the heart which is uncovered, and which is in contact with the walls of the chest. But, on the other hand, when opportunity offers it has been found that the sound in question is heard over the aortic valves more distinctly than in any other situation. A remarkable case of this kind has been related by M. Cruveilhier, who was invited to see an infant just born presenting a complete case of ectopia of the heart. He says, "On examining the heart thus exposed, both sounds were

distinctly heard over the base, and *not* at the apex." He remarks, "We must not forget that one element of the sound, that produced by the shock of the heart against the chest-wall, is absent."*

Another point of interest is that which has already been alluded to in the case of typhus fever, as described by Dr. Stokes, namely, that the sound disappears last over the semi-lunar valves, and also that the returning sound is first heard in the same situation.

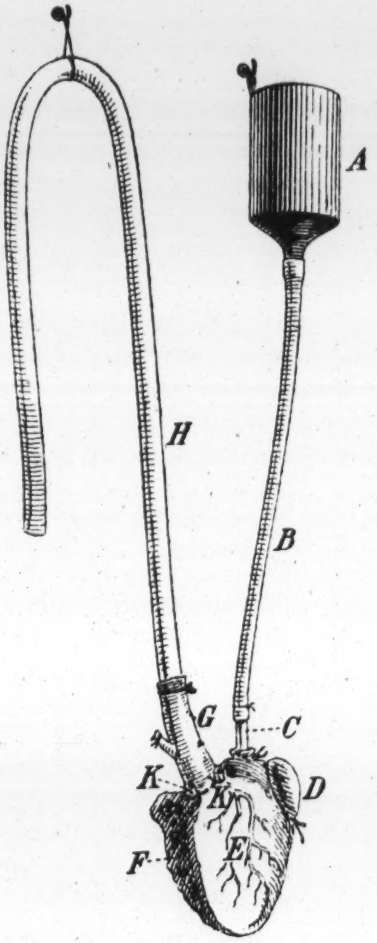
It is also an object of great interest to compare the *characters* of the two sounds in relation to the seat of their origin at the semi-lunar valves. In the first sound we have the character of propulsive force and sustained action, softer and more prolonged than the second sound, which is sudden, sharp, and short, as if produced by an abrupt mechanical disturbance. These distinctions, which may be readily recognised in various degrees by careful observation, serve to convince us that the sounds are both produced at the same point: at the semi-lunar valves, each by its own single and simple agency.

D. Lastly, sounds resembling the first (and second) sound of the heart can be produced artificially in accordance with the view contained in the preceding communication.

The experiment is thus made: a sheep's heart of good size (or that of a calf) may be used. It must be carefully cleared from pericardium, leaving the large vessels and pulmonary veins as far as possible intact. The orifices of the pulmonary veins must be laid into one, so as to permit a sufficient opening into the left auricle through which to divide the attachments of the mitral cusps and the musculi papillares in the left ventricle, taking care in doing this not to injure the aortic segments when detaching the cusp that lies next them. The coronary artery must next be ligatured, and also the innominate artery where it springs from the aortic arch. The right auricle and ventricle should be removed. Through the opening made by laying into one the orifices of the pulmonary veins a bone nozzle should be passed. It is well also to ligature the auricular appendix and any points from which water may issue when the ventricle is filled. To the posterior orifice of the nozzle rubber tubing should be attached, communicating with a source of water supply, placed on a higher level; and another portion of rigid gutta-percha tubing, about 3 feet long, should be introduced into the aorta (see diagram, fig. 1). If now the ventricle be filled with water by means of the tube in the left auricle, the water will of course pass into the ventricle and thence up the aorta, a portion of the water resting upon and closing the aortic sigmoid valves.

* 'Gazette Médicale,' p. 488 (1841).

Fig. 1.—Diagram to illustrate Sir Richard Quain's theory of the mechanism causing the first sound of the heart.



A. Receptacle for water-supply connected by means of tubing B with bone nozzle C tied into the left auricle.

D. The left auricular appendix ligatured after escape of all air from the heart chambers.

E. The left ventricle of the heart.

F. The site of the right chambers of the heart which were removed after ligature of the coronary arteries KK.

H. Tubing connected with the aorta G and containing water representing the aortic column of blood.

If further the ventricle be compressed periodically in imitation of systolic contraction, and allowed to relax in imitation of diastolic relaxation, a sound closely resembling the first sound of the heart will be produced when water is propelled from the ventricle into the aorta, and another closely resembling the second sound when propulsive movement ceases, and the sigmoid valves again close under the superincumbent weight of water in the aortic tubing. As the fluid

rises in the gutta-percha tube, the pressure on the valves increases, and the sound becomes more marked; when the fluid on the other hand diminishes, the sounds become less distinct. If the heart be placed horizontally, the sounds become wobbly. The terminal piece of small diameter of a binaural stethoscope gently placed over the aorta at its commencement is most suitable for observing the cardiac sounds in this experiment.

The experiment here described, when first suggested by me, was submitted, with the assistance of Dr. Sibbald, to my lamented friend the late Dr. Sharpey, and to Sir James Paget, who were quite satisfied that with the increase or diminution of the column the sounds closely resembled those of the heart in man, and that they became more or less distinct in proportion to the quantity of fluid contained in the tube.

In conclusion, I may say that I was moved to undertake and continue this inquiry by a desire to obtain a solution of what seemed to be an insoluble problem, and also by a belief that a correct explanation of the cause of the first sound of the heart would be of practical value in the study of the clinical phenomena of diseases of this organ.

I would wish further to add that the explanation of the cause of the first sound of the heart given in this communication, being so different from that hitherto accepted, may seem calculated to create difficulties in the diagnosis of valvular diseases of the heart. Closer consideration will show, however, this not to be the case, but that, like all accurate knowledge, it will be found to simplify, and not to confuse. It will afford an explanation of the relations of certain morbid phenomena which are at present unintelligible, such, for example, as that a systolic murmur may be heard at the apex whilst the first sound is audible at the base free from murmur; and it will serve to encourage a closer study of the relation between muscular contraction of the walls of the heart and the tension of the vessels of the system.

"Mathematical Contributions to the Theory of Evolution. On the Relative Variation and Correlation in Civilised and Uncivilised Races." By Miss ALICE LEE, Bedford College, and KARL PEARSON, M.A., F.R.S., Professor of Mathematics and Mechanics, University College, London. Received April 9,—Read June 3, 1897.

1. The following numerical data were calculated in the hope of reaching some general ideas on the comparative variation and com-

parative correlation in the case of civilised and uncivilised races, and further of determining, if possible, any general law connecting relative sexual variation and relative sexual correlation with the degree of civilisation, and so with what is probably inversely proportional to the degree of civilisation, namely, the intensity of natural selection.

The following two principles seem to flow from a study of variation in the organs of man :—*

(*a*) Civilised man is more variable than uncivilised man.

(*b*) There is a greater equality of variation for the two sexes in uncivilised than in civilised races. Civilised woman appears, on the whole, to be slightly more variable than civilised man.

Both these principles are in accordance with the intensity of the struggle for existence—and the amount, consequently, of natural selection—being greater for uncivilised than for civilised races, and, further, greater for men than for women in the latter races.

The problem of correlation is, however, of a less simple character. While the action of selection can be shown theoretically to reduce variation, it by no means follows that it reduces correlation. Indeed, selection may increase, decrease, or reverse correlation at the very same time as it is reducing variation.† We have then the following problems to guide us in our treatment of actual statistics :—

(*a*) Is correlation more intense among civilised than among uncivilised races ?

(*b*) How does the relative correlation of the sexes differ in civilised and uncivilised races ?

(*c*) Is there any marked prepotency of either sex in the matter of correlation ?

These are the problems which the present calculations were designed, not to definitely solve, but to illustrate.

2. Unfortunately, measurements on living members of uncivilised races are not very numerous, nor for the purposes of correlation generally very satisfactory. There are numerous measurements of skulls and some of bones, but the measurements for each individual race are, from the mathematical standpoint, but few in number. Hence, in the case of each determination of a coefficient of variation or a coefficient of correlation, the probable error will be large. It

* See 'Variation in Man and Woman,' by K. Pearson : "The Chances of Death," vol. 1, pp. 256—377, where some 155 cases of human variation for both sexes are dealt with.

† The theoretical discussion of the relation of selection to correlation forms the subject of a memoir nearly completed. Actually there is some evidence to show higher correlation among the working classes than among the middle and upper classes of a civilised community.

is thus impossible to lay much stress on any individual relative difference; we are bound to consider only the *general trend* of our results. In order, however, to make the probable errors as small as possible it is desirable in the case of correlation to make the coefficients as large as possible. Now, for measurements on the skull, the correlation is small, but, for the long bones, it is large. Hence, the latter will give better results for comparative purposes, if the series be smaller than the former. They have accordingly been selected for the present investigation. In all cases the standard deviations, coefficients of variation, and coefficients of correlation have been calculated *without grouping* by means of the formulæ given in a memoir on "Regression, Heredity, and Panmixia."* The standard deviations and coefficients of correlation had their errors determined by the formulæ of the same memoir (pp. 266 and 275); the coefficients of variation have for probable error, if n be the number of measurements in the series, and V the coefficient,

$$0{\cdot}6745\,\frac{v}{\sqrt{2n}}\left[1+2\left(\frac{v}{100}\right)^2\right]^{\frac{1}{2}},$$

or sensibly, $0{\cdot}6745\,\dfrac{v}{\sqrt{2n}}$, since $v/100$ is of the order 0·05, for our present investigations. It will thus be seen that the probable error of a coefficient of variation can, like that of a standard deviation, amount to a considerable percentage of its value if n be small. For example, if $n = 23$ (case of the Aino women) it can amount to about 10 per cent.; while for $n = 50$ (case of the French men or women) it will amount to 6·7 per cent. The probable errors of the standard deviations will also in the two cases amount to 10 and 6·7 per cent. respectively. On the other hand, if n be as small as 23, the probable error of the coefficent of correlation will only be 2·2 per cent. of its value if r be as large as 0·9, and only 6·56 per cent. of its value if $r = 0{\cdot}75$. On the other hand for $n = 50$ the probable errors of r for the two cases of $r = 0{\cdot}9$ and 0·75 are respectively 1·5 and 4·45 per cent. It thus appears that if correlation be large, or even fairly large, it can be determined with far less probable error than variation. The results for correlation are thus likely in the present case to be more significant than those for variation.

3. As to the material itself the French measurements are taken from a work by Dr. E. Rollet: 'De la Mensuration des Os longs des Membres,' published in the 'Bibliothèque d'Anthropologie Criminelle,' Lyons, 1889. The bodies from which the bones were taken having passed through the post-mortem room, the sex was known without question. The right member was always taken

* 'Phil. Trans.,' A, vol. 189 pp. 265 and 277.

except in the few cases where its length was not recorded. In these cases in order not to diminish the already small series, the left member was taken. Of the two measurements of the femur given by Rollet the longer was taken.

The measurements of the Aino bones were taken from a work by Koganei in the 'Mittheilungen aus der Medicinischen Facultät der K. Japanischen Universität,' Bd. 2—Tables, Tokio, 1894; and the same rule observed. The Japanese returns, while more extended in the variety of bones measured than the French, yet contain fewer individuals, and in many respects do not seem to us so reliable as the latter.* In both cases the maximum length of femur, humerus, radius, ulna, fibula, and clavicle (F_1, H, R_1, U, Fib., and Cl.) were measured. For the Aino we have in addition to the lengths (R_2 and T_2) of the radius and tibia from centre to centre, the length (F_2) of head and neck of femur, and the scapular index (Sc.) or ratio of breadth to length of scapula. The Aino measurements give the maximum length of the tibia (T_1) as well as the length from centre to centre. The French measurements give the length of the tibia, excluding the spine and including the malleolus (T_3). In order that our results might be of service to Mr. E. Warren, who is dealing at length with the Libyan long bones, the radius correlations for the Aino were calculated for R_2 and not for R_1, as they are in the case of the French. The results for French and Aino are not strictly comparable in this case. The same holds for the measurement of the tibia. Probably Rollet's T_3 measurement corresponds more closely to Koganei's measurement No. 13 (T_1) than to his No. 14 (T_2). Both T_1 and T_3 are greater, T_2 is less than the fibula in length.

We have dealt with the Aino T_2 instead of T_1, however, for the purpose of comparison with the Libyans.

The following tables give the chief results :—

* One female humerus (No. 42) was noticed to be very discordant. As it would be excluded as a "discordant observation" by Chauvenet's criterion it was rejected; it was afterwards found to be described in the text as deformed. The male humerus (No. 31) should be 279 and not 379 cm. long. The female ulna, No. 4, (270 cm.) has been rejected. It is out of all proportion to the corresponding humerus, as judged by the whole range of humerus ulna measurements we have come across. Yet notwithstanding its disproportionately large size, Koganei describes it (p. 127) as much bent. The male fibula, No. 19, is excessive as compared with either femur or tibia. There did not, however, seem sufficient warrant for rejecting it. Its being a misprint or mismeasurement would account for the low values obtained for the ♂ Aino correlation of fibula with femur and tibia. Without it the fibula-tibia correlation = 0·9492.

Table I.—Variation ♂.

(Absolute Measurements in Centimetres.)

Organ.	Aino.				French.			
	No.	Mean.	S.D.	C. of V.	No.	Mean.	S.D.	C. of V.
Femur, F_1	44	40·770 ±0·193	1·898 ±0·136	4·655 ±0·336	50	45·228 ±0·226	2·372 ±0·160	5·425 ±0·354
Femur, F_2	41	6·790 ±0·041	0·393 ±0·029	5·788 ±0·427	—	—	—	—
Tibia, T_1	38 (T_1)	33·895 ±0·183	1·668 ±0·129	4·921 ±0·381	50 (T_3)	36·806 ±0 172	1·799 ±0·121	4·888 ±0·330
Tibia, T_2	40	31·740 ±0·168	1·577 ±0·119	4·968 ±0·375	—	—	—	—
Fibula, Fib.	32	33·263 ±0·189	1·585 ±0·134	4·765 ±0·403	50	36·272 ±0·172	1·801 ±0·121	4·965 ±0·335
Humerus, H.	45	29·502 ±0·135	1·343 ±0·095	4·552 ±0·322	50	33·010 ±0 147	1·538 ±0·104	4·659 ±0·314
Radius, R_1	39	22 913 ±0·121	1·117 ±0·086	4·875 ±0·375	50	24·394 ±0·112	1·170 ±0·079	4·796 ±0·323
Radius, R_2	39	21·549 ±0·114	1·060 ±0·081	4·919 ±0·376	—	—	—	—
Ulna, U.	38	24·603 ±0·121	1·109 ±0·086	4·508 ±0·350	50	26·052 ±0·125	1·309 ±0·088	5·025 ±0·339
Clavicle, Cl.	35	14·57 ±0·088	0·776 ±0·062	5·326 ±0·426	—	—	—	—
Scapular Index, Sc.	33	65·56 ±0·487	4·15 ±0·344	—	—	—	—	—

Table II.—Variation ♀.

(Absolute Measurements in Centimetres.)

Organ.	Aino.				French.			
	No.	Mean.	S.D.	C. of V.	No.	Mean.	S.D.	C. of V.
Femur, F_1.....	25	38·204 ±0·215	1·594 ±0·152	4·170 ±0·398	50	41·570 ±0·215	2·255 ±0·152	5·425 ±0·366
Femur, F_2.....	24	6·200 ±0·037	0·269 ±0·026	4·339 ±0·422	—	—	—	—
Tibia, T_1.....	22 (T_1)	31·859 ±0·206	1·434 ±0·146	4·501 ±0·458	50 (T_3)	33·444 ±0·178	1·863 ±0·126	5·571 ±0·376
Tibia, T_2.....	24	29·800 ±0·217	1·576 ±0·153	5·289 ±0·515	—	—	—	—
Fibula, Fib.....	20	31·210 ±0·243	1·612 ±0·172	5·165 ±0·551	50	33·030 ±0·175	1·837 ±0·124	5·562 ±0·375
Humerus, H.....	27	27·7185 ±0·164	1·242 ±0·116	4·481 ±0·419	50	29·766 ±0·145	1·525 ±0·103	5·123 ±0·346
Radius, R_1.....	24	21·083 ±0·139	1·009 ±0·098	4·786 ±0·466	50	21·486 ±0·104	1·095 ±0·074	5·096 ±0·344
Radius, R_2.....	24	19·929 ±0·136	0·991 ±0·096	4·973 ±0·484	—	—	—	—
Ulna, U.....	21	23·071 ±0·173	1·176 ±0·122	5·095 ±0·530	50	23·210 ±0·110	1·154 ±0·078	4·972 ±0·335
Clavicle, Cl.....	20	13·235 ±0·137	0·908 ±0·097	6·861 ±0·732	—	—	—	—
Scapular Index, Sc.	20	68·64 ±0·538	3·57 ±0·381	—	—	—	—	—

Scapular Index, Sc.	20	68·64 ±0·538	3·57 ±0·381	—	—	—	—	—

Table III.—Correlation ♂ and ♀.*

Organs.	♂.				♀.			
	Aino.		French.		Aino.		French.	
	No.	*r*.	No.	*r*	No.	*r*.	No.	*r*.
Femur and tibia, F_1. and T_2.....	40 (T_2.)	0·8266 ±0·0260	50 (T_3.)	0·8058 ±0·0261	24 (T_2.)	0·8457 ±0·0299	50 (T_3.)	0·8904 ±0·0148
Femur and humerus, F_1. and H...	42	0·8584 ±0·0208	50	0·8421 ±0·0212	23	0·8722 ±0·0254	50	0·8718 ±0·0173
Femur and radius, F_1. and R_2.....	37 (R_2.)	0·7891 ±0·0328	50 (R_1.)	0·7439 ±0·0341	22 (R_2.)	0·7039 ±0·0593	50 (R_1.)	0·7786 ±0·0296
Tibia and humerus, T_2. and H.....	39 (T_2.)	0·7447 ±0·0257	50 (T_3.)	0·8601 ±0·0188	22 (T_2.)	0·7277 ±0·0547	50 (T_3.)	0·8180 ±0·0244
Tibia and radius, T_2. and R_2.....	35 (T_2. and R_2.)	0·8655 ±0·0216	50 (T_3. and R_1.)	0·7804 ±0·0294	21 (T_2. and R_2.)	0·7452 ±0·0525	50 (T_3. and R_1.)	0·8053 ±0·0261
Clavicle and humerus, Cl. and H...	34	0·4393 ±0·0855	—	—	20	0·6309 ±0·0768	—	—
Clavicle and scapula, Cl. and Sc...	30	0·1738 ±0·1177	—	—	17	0·1191 ±0·1601	—	—

Table III.—Correlation ♂ and ♀—*continued.**

Organs.	♂.				♀.			
	Aino.		French.		Aino.		French.	
	No.	*r*.	No.	*r*.	No.	*r*.	No.	*r*.
Humerus and radius, H. and R_2...	39 (R_2.)	0·7763 ±0·0338	50 (R_1.)	0·8451 ±0·0208	22 (R_2.)	0·7386 ±0·0526	50 (R_1.)	0·8515 ±0·0200
Tibia and fibula, T_2. and Fib.....	37 (T_2.)	0·8882 ±0·0190	50 (T_3.)	0·9575 ±0·0057	18 (T_2.)	0·9670 ±0·0074	50 (T_3.)	0·9803 ±0·0027
Femur and fibula, F_1. and Fib.....	32	0·8310 ±0·0284	50	0·8193 ±0·0243	19	0·8890 ±0·0242	50	0·9106 ±0·0120
Humerus and ulna, H. and U.....	38	0·7653 ±0·0360	50	0·7669 ±0·0312	20	0·7479 ±0·0532	50	0·8600 ±0·0188
Radius and ulna, R_2. and U.....	38 (R_2.)	0·9780 ±0·0056	50 (R_1.)	0·8767 ±0·0166	20 (R_2.)	0·9754 ±0·0525	50 (R_1.)	0·9244 ±0·0102

* The probable error of *r*, the coefficient of correlation, is calculated from the formula given in the memoir cited p. 345. That formula is based on the assumption that the probable error of *r* is uncorrelated with the probable errors of the variations of the two correlated organs, σ_1, σ_2. If this correlation be taken into account, the formula for *r* is somewhat modified, and the values calculated must be slightly increased. The probable error of *r* used is actually that of an array of *r*'s corresponding to the given values of σ_1 and σ_2.

Table IV.—Sexual Ratios: ♂ to ♀.

Organ.	Means.		Variations.	
	Aino.	French.	Aino.	French.
Femur, F_1.	1·067	1·087	1·116	1·000
Femur, F_2.	1·095	—	1·333	—
Tibia, T_1.....	1·063	—	1·093	—
Tibia, T_2.....	1·065	(T_3) 1·101	0·939	(T_3) 0·877
Fibula, Fib.....	1·066	1·098	0·922	0·893
Humerus, H.	1·064	1·109	1·016	0·909
Radius, R_1.....	1·087	1·135	1·019	0·941
Radius. R_2.....	1·081	—	0·989	—
Ulna, U.....	1·066	1·122	0·885	1·011
Clavicle, Cl.....	1·101	—	0·776	—
Scapular Index, Sc.	0·955	—	1·162	—
Mean of F_1., T_2., Fib., H., R_2., and U.	1·068	1·109	0·978	0·939
Mean of all measurements	1·065	—	1·023	—

Table V.—Sexual Ratios: ♂ to ♀.

Correlations.

Organs.	Aino.	French.
Femur and Tibia, F_1. and T_2.	0·977	(T_3.) 0·905
Femur and Humerus, F_1. and H.	0·984	0·966
Femur and Radius, F_1. and R_2.	1·121	(R_1.) 0·957
Tibia and Humerus, T_2. and H.	1·023	(T_3.) 1·051
Tibia and Radius, T_2. and R_2.	1·161	(T_3, R_1) 0·969
Clavicle and Humerus, Cl. and H.....	0·696	—
Clavicle and Scapular Index	1·459	—
Humerus and Radius, H. and R_2.....	1·051	(R_1.) 0·992
Tibia and Fibula, T_2. and Fib.	0·918	(T_3.) 0·977
Femur and Fibula, F_1. and Fib.	0·935	0·900
Humerus and Ulna, H. and U.	1·023	0·892
Radius and Ulna, R_2. and U.....	1·003	(R_1.) 0·948
Mean (excluding Clavicle and Scapular Index correlations)	1·020	0·956
Mean (including these)	1·029	—

Table VI.—Racial Ratios: French to Aino.

Organ.	Means.		Variations.	
	♂.	♀.	♂.	♀.
Femur, F_1...	1·109	1·088	1·165	1·301
Tibia, T_2. or T_3.....	1·159	1·122	0·983	1·053
Fibula, Fib.	1·090	1·058	1·042	1·077
Humerus, H.....	1·119	1·074	1·023	1·143
Radius, R_1. or R_2.....	1·132	1·078	0·975	1·025
Ulna, U.....	1·059	1·006	1·115	0·976
Means.....	1·111	1·071	1·050	1·096

Rate of progression of males over females in means = 1·037.

Rate of progression of females over males in variations = 1·043.

Table VII.—Racial Ratios: French to Aino.

Correlations.

Organs.	♂	♀
Femur and Tibia, F_1. and T_2. or T_3	0·975	1·053
Femur and Humerus, F_1. and H.	0·981	1·000
Femur and Radius, F_1. and R_2. or R_1.....	0·943	1·106
Tibia and Humerus, T_2. or T_3., and H.	1·155	1·124
Tibia and Radius, T_2. and R_2. or T_3. and R_1...	0·902	1·081
Humerus and Radius, H. and R_2., or R_1.....	1·089	1·152
Tibia and Fibula, T_2. and Fib.....	1·078	1·014
Femur and Fibula, F_1. and Fib.	0·986	1·024
Humerus and Ulna, H. and U.	1·002	1·150
Radius and Ulna, R_2. or R_1. and U	0·896	0·948
Means	1·0007	1·065

Rate of progression of females over males in correlation = 1·065.

5. Now the tables illustrate at once how little can be judged for the problem of variation from any *single* measurement when the series are so small, and the measurements do not entirely correspond. We are forced to argue only from the general trend of the whole group. On the other hand, with the exception of the clavicle and scapular index, the probable errors in the correlation are sufficiently small for fairly good inferences to be drawn.* We will deal first with the general problems with regard to sex, and then with those bearing on race.

6. *Sexual Ratios.*—In all cases the sexual ratio for the mean length of any bone is greater than unity. Grouping both French and Aino together, we have 17 series giving a mean sexual ratio for the long bones in civilised and uncivilised man of 1·088. This agrees well with the value 1·089 determined for a series of rather different contents in the paper on 'Variation in Man and Woman' (*loc. cit.*, p. 374) referred to above. Turning to sexual ratio in variation, we find, for French and Aino together, the value 0·976. The value as

* So far as these relate to the nature of the correlation between individual long bones, we have placed our results at the disposal of Mr. E. Warren, who is dealing with the long bones of the Libyan (?) skeletons.

determined from about 155 series (*loc. cit.*, p. 373) was 0·973 ± 0·007. A special study of the long bones thus seems to confirm the result previously reached, that woman is, on the whole, slightly more variable than man. Lastly, turning to the sexual ratio in correlation, we find that in 14 out of 22 cases, woman is more highly correlated than man, the mean sexual ratio in correlation being 0·992. So far, then, as the present results go, they seem to indicate that man, while larger, is less variable, and less highly correlated than woman.

When we take, however, race into consideration, there are additional factors which somewhat modify these conclusions. In the first place there is, of course, no suggestion that the modern French are lineal descendants of the Aino, but it does not seem an improbable supposition that the French have at one time passed through a stage somewhat similar to that primitive condition in which we now find the Aino. Assuming this for the purpose, at any rate, of comparison, we find that the transition from the uncivilised to the civilised condition is accompanied by well-marked changes in the sexual ratios. They are as follows :—

(i.) Man tends to gain in size on woman.
(ii.) Woman tends to gain in variability on man.
(iii.) Woman tends to gain in correlation on man.

In other words, primitive man and woman are more nearly equal in size, variability, and correlation than highly civilised man and woman. Relative to woman, man's gain in size has been accompanied by a loss in variability and in the correlation of his parts. So far as size and variability are concerned, these results are in accordance with the far wider series of measurements dealt with in the paper on 'Variation in Man and Woman.' The result as to correlation was there suggested (*loc. cit.*, p. 375) on very slight evidence. Before we make any suggestion as to the bearing of these results on natural selection it will be well to examine the racial ratios.

7. *Racial Ratios.*—In means both ♂ and ♀ French are in every measurement larger than ♂ and ♀ Aino, but the difference is greater for males than females. In variations the ♂ and ♀ French have progressed in nearly all cases on the ♂ and ♀ Aino (the tibia in the male and the ulna in the female are apparently exceptions). But while the females have progressed less rapidly than the males in absolute size, they have progressed more rapidly in variation. Lastly, in correlation the French males have more correlation than the Aino males in four organs, and less in six organs, the general mean result being almost equality of correlation. On the other hand, the French females are only inferior to the Aino in one case, and the final result is a very sensible progress in correlation. The general

conclusion would then be that, with increased civilisation, absolute size* and variation tend to increase; while correlation, to judge by the males, is stationary; to judge by the females, tends to increase.

It will be found somewhat difficult to reconcile these results with any simple applications of the principle of natural selection. In the first place increased variation undoubtedly suggests a lessening of the struggle for existence, and there can be no question that this increase has gone on among civilised races (see 'Variation in Man and Woman'). The lessening of the struggle has probably been greater for woman than man; hence the principle of natural selection might help to explain the preponderance of variability in civilised woman. The increase in size with civilisation seems, on the average, also incontestable. But is it the effect of lessening the struggle for existence? The possibilities may, perhaps, be summed up as follows:—

(*a*) The civilised races may have survived owing to their superior size. It may be a result of the struggle in the past. To this must be objected that the increase of size appears to be a progressive change still going on, and yet increase of variation should show a lessening struggle for existence.

(*b*) The effect of suspending natural selection may be to increase size. This would be a blow for panmixia, for we might naturally have expected a regression to the smallness of the more primitive races. It would leave unexplained the apparently smaller progress of women as compared with men, for in their case we might argue from the variation that the struggle for existence is relatively less than in the case of man.

(*c*) The larger size of the civilised races may be due to better food supply and better physical training: in short it may be due, not to evolution, but to better conditions of growth. This hypothesis does not involve the assumption that acquired characters are inherited. Diminish the food supply and abolish physical training and the size would sink to the level at which natural selection had left it. Physical training in civilised races being usually more adequate in the case of man than of woman would, perhaps, explain why man has progressed more rapidly in size than woman. It seems impossible, taking variation as a measure of the intensity of selection, to reconcile the relative increases in size of man and woman with any direct effect of natural selection.

* This is only *generally* true, not in every individual case. The French femur is longer than that of the Aino, of neolithic man, and of the ancient inhabitants of the Canary Islands. On the other hand, the French femur appears to be slightly less than the Libyan, although the humerus is somewhat greater. The French women appear in all long bones less than the Libyan women.

8. To sum up, then, the following results seem *suggested* by these measurements.

(i.) Civilised man has progressed generally on primitive man in size, variation, and correlation.

(ii.) This progression can hardly be accounted for by increased selection (because of the increased variation), nor by decreased selection (because it is inconsistent with the relative changes in male and female size). It might possibly be accounted for by decreased selection and improved physical conditions.

(iii.) Woman is more variable than man in civilised races.

(iv.) Woman is more highly correlated than man in civilised races.

(v.) In uncivilised races the sexes are more nearly equal in the matter of size, variation, and correlation than in the case of civilised races.

(vi.) It is impossible to say that civilised woman is nearer to the primitive type than civilised man, for while civilised man differs more from the primitive type than civilised woman, so far, probably, as absolute size is concerned, he has made only about half her progress in variation, and hardly any progress at all in correlation.

(vii.) The causes (*e.g.*, lessening of selection) which tend to increase variation may also increase correlation. In other words, the intensity of the struggle for existence is not necessarily a measure of the intensity of correlation.*

The measurements made by Mr. Warren on the Libyans, the results of which he has kindly favoured us with, are, on the whole, fairly in accordance with the above conclusions. He finds for the

Mean of the sexual ratio of the means	1·092
„ „ „ the variations .	1·028
„ „ „ the correlations	1·068

The corresponding quantities for the French are: 1·109, 0·939, 0·956, or, we concluded, that in passing from uncivilised to civilised peoples, from Libyan to French, the men gain on the women in size—here very slightly, and the women gain upon the men very markedly in variation and correlation.

These results are merely suggestions, but they may possibly serve to emphasise the importance of a careful measurement of the long bones of, say, 100 members of both sexes for a series of civilised and uncivilised races. In the former case at least there does not appear to be any real difficulty, except the need of co-operation, in obtaining

* The mathematical theory of selective correlation shows that the close selection of an organ, say the femur, may actually tend to reduce the correlation between two other organs, say the humerus and the radius.

measurements similar to those of M. Rollet for both English and Germans. The value of such statistics for comparative purposes would be very great.

"On the Nature of the Contagium of Rinderpest. Preliminary Communication." By ALEXANDER EDINGTON, M.B., F.R.S.E., Director Colonial Bacteriological Institute, Cape Colony. Communicated by Sir JAMES CRICHTON BROWNE, M.D., LL.D., F.R.S. Received March 22,—Read June 3, 1897.

In the following pages it is proposed to communicate to the Royal Society the results of experiments made in South Africa on the infectivity of the blood of animals affected with Rinderpest. The experiments were all made on cattle kept under conditions in which accidental spontaneous infection could with certainty be excluded. These experiments had been concluded in 1896, before the arrival of Dr. R. Koch in South Africa, and their results had been communicated to him on his arrival.

1. The blood of an animal ill with rinderpest, when taken during the febrile stage or previous to death, and injected subcutaneously or intravenously into healthy cattle, produces the typical disease—rinderpest, provided the blood is prevented from coagulating.

2. The onset of coagulation and actual coagulation of the blood exert a marked destructive influence on the virulence of such blood.

3. The best method of obtaining virulent blood is to draw it aseptically from the jugular vein of an animal ill with rinderpest, and to mix it immediately with a 1 per cent. solution of citrate of potash, the latter previously well sterilised, in the proportion of 2—3 parts of blood to 1 part of citrate of potash solution. Such blood, as has been shown, remains fluid.

4. This citrate of potash mixture of blood proves virulent in the first few days, generally not exceeding six days; after six days' keeping the virulence becomes rapidly weakened, so that after nine days the blood mixture is altogether inert.

5. Admixture of glycerine to citrate blood does not *cæteris paribus* interfere with the virulence of such blood. Glycerine added to fresh blood does interfere with the virulence of the latter on account of the coagulation of the blood.

6. The nasal mucus of an infected animal when used fresh and rubbed into the nostrils of normal cattle, produced in all instances typical rinderpest. We have never had a single failure in attempting to produce the disease by this means. By keeping the nasal mucus, even for a few hours, its virulence becomes markedly less.

7. The condition of marked swelling of the lymphatic glands is one of, if not indeed the most evident pathognomic feature of the disease. The contagium exists as a primary infection in the lymphatic glands.

8. A very mild attack of rinderpest, such as is produced by injection of blood of greatly decreased virulence, does not convey absolute immunity, this latter being produced in proportion to the severity of the attack through which the animal had passed primarily. An animal seemingly affected may have a relapse of the disease, which may go on to fatal issue or be mild in type, leading to recovery. Animals in the latter case always acquire immunity of a high degree.

"On the Dielectric Constants of certain Organic Bodies at and below the Temperature of Liquid Air." By JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London. Received June 29, 1897.

Continuing the researches on which we are engaged on the Dielectric quality of matter at very low temperatures, we have examined a number of organic bodies and measured their dielectric constants at and above the temperature of liquid air.

The apparatus and arrangements for effecting this measurement are described in a former communication by us on the same subject.*

The frequency of the reversals of the electromotive force used in charging the condenser in the experiments here described was, as before, 120. The condenser used was the gilt cone condenser described in the communication mentioned.

In another paper† we have given the results obtained by us on the measurement of the dielectric constants of glycerine, ethylene dibromide, and nitrobenzol at very low temperatures, and shown that in the case of glycerine the very high value of the dielectric constant possessed by this body, above $-100°$ C., is reduced, on cooling to $-185°$ C., to a value not far from 3·0. We have also shown that a similar great reduction in the value of the dielectric constant takes place in the case of ethyl alcohol when frozen at $-185°$ C.‡

* See Fleming and Dewar, "On the Dielectric Constants of Certain Frozen Electrolytes at and above the Temperature of Liquid Air," 'Roy. Soc. Proc.,' vol. 61, p. 299.

† See Fleming and Dewar, "On the Dielectric Constants of Ice, Glycerine, Ethylene Dibromide, and Nitrobenzol at and above the Temperature of Liquid Air," 'Roy. Soc. Proc.,' vol. 61, p. 316.

‡ See Dewar and Fleming, "Note on the Dielectric Constants of Ice and Alcohol at very Low Temperatures," 'Roy. Soc. Proc.,' vol. 61, p. 2.

We have now examined, in addition, the following organic bodies, viz., *formic acid*, *methyl alcohol*, *amyl alcohol*, *acetone*, *chinoline*, *tetra-ethylammonium hydrate*, and *carbolic acid*, and we find that these bodies have, in the liquid or solid condition and at temperatures above −100° C., high, or very high, dielectric values, but all have their dielectric constants reduced to a value not far from 2·5 when they are cooled to the temperature of liquid air.

In addition, we have measured the dielectric constants of *castor oil* and *olive oil*, and find that they are reduced in value when these oils are cooled to −185° C. We have also measured the dielectric constants of *bisulphide of carbon*, *ether*, *ethyl nitrate*, *solution of sulphur in bisulphide of carbon*, and *absolute alcohol* with 10 per cent. of metallic sodium in it. We find in all these cases that there is a reduction in the value of the dielectric power, large or small, these bodies possess at normal temperatures. These experiments further confirm the view that the presence of the radicles *hydroxyl* (HO), *carbonyl* (CO), and the organic acid radicle (COOH) always produce relatively high dielectric values in the bodies containing them when these bodies are in the liquid condition or at temperatures not very far below their freezing points. When these bodies are, however, frozen and cooled to the temperature of liquid air, all of them suffer a reduction in dielectric power, and their dielectric constants are reduced to values not far from 2·5, or between 2·5 and 2·6, at the temperature of −185° C. or a little below. The action of the low temperature is thus to annul the effect of the radicle.

We have not found, so far, any exception to the rule that pure organic bodies have a dielectric constant of from two to three times that of vacuous space when they are cooled to the temperature of boiling liquid air, no matter what value their constant may have when in the liquid condition.

The following tables give the numerical results, and the observations are delineated graphically in the chart in fig. 1.

All the tables are headed by a statement giving the galvanometer scale deflection in centimetres when the condenser has air as dielectric. This is to be understood as the constant, for the time being, representing the capacity of the condenser when its dielectric is gaseous air at normal pressure and temperature. The numbers in the columns headed "Dielectric constant," under each substance are obtained by dividing the number in the second column of the tables, headed "Mean galvanometer deflection," by the above air capacity constant, after having applied a correction to the galvanometer reading for the charging voltage and for the capacity of the leads. The numbers headed "Mean galvanometer deflection," when corrected for voltage and the capacity of the leads and vibrator, represent, to the same scale as the corrected air capacity constant, the capacity

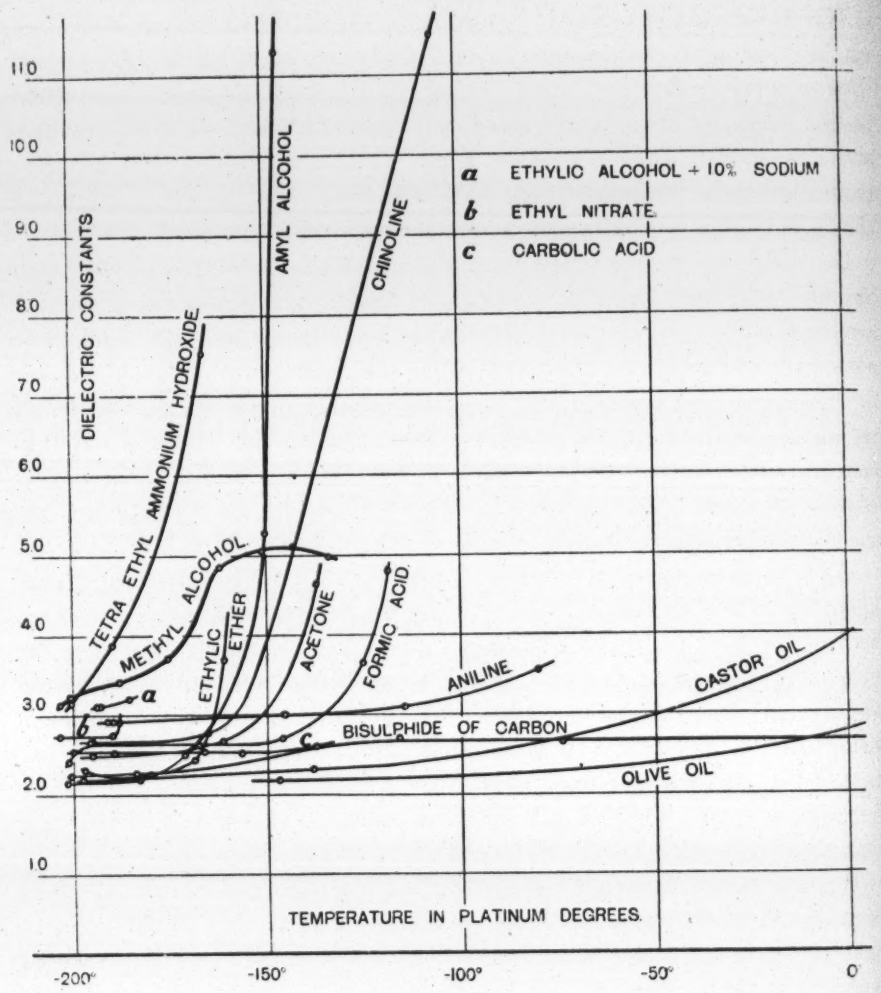


FIG. 1.—Curves showing the Variation in Dielectric Constant with Temperature of Various Organic Bodies.

of the condenser when the particular substance mentioned was being used as dielectric. The temperatures are given in platinum degrees, as explained in our other papers to which reference is made. The last column in the tables gives the electromotive force, in volts, used to charge the condenser. The frequency of reversals was in all cases 120 per second.

The observations were stopped in each case at that temperature at which any sensible true conductivity made its appearance in the dielectric, and this, as we have already shown, takes place in the case of many bodies quite suddenly, and at temperatures far below their melting point. Thus it is impossible to carry up the dielectric constant measurement, by the method we employed, of formic acid or methyl alcohol much above about $-120°$, because at that point

sensible conductivity begins. There is no difficulty, however, in carrying out the measurement in the case of carbon bisulphide, castor oil, or olive oil, over the whole range from $-200°$ up to $+15°$. At the liquid air temperature all the organic bodies in the following tables are, however, practically perfect insulators.

The following tables give the figures of observation :—

I. *Methyl Alcohol* (CH_4O).

Corrected galvanometer deflection when the condenser had air as dielectric = 3·96 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−203·4	13·2	3·13	Condenser charged
−200·8	13·5	3·23	to 102 volts.
−188·0	14·2	3·41	
−175·4	15·4	3·71	
−161·9	19·75	4·85	
−151·0	3·71	5·05	Condenser charged
−143·0	3·75	5·10	to 18·3 volts.
−134·0	3·78	4·98	

II. *Amyl Alcohol* ($C_5H_{12}O$).

Corrected galvanometer deflection when the condenser had air as dielectric = 4·24 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−204·0	10·4	2·40	Condenser charged to 98·2 volts.
−201·8	1·89	2·14	Condenser charged to
−183·0	1·93	2·19	18·6 volts.
−171·5	2·05	2·50	
−150·2	4·30	5·28	
−146·2	9·00	11·3	
−141·4	19·50	24·7	
−138·8	2·10	34·4	Condenser charged to
−133·3	2·61	43·0	1·43 volts.
−127·7	2·55	42·0	
−123·8	2·50	41·0	

III. *Formic Acid* (CH_2O_2).

Corrected galvanometer deflection when the condenser had air as dielectric = 3·97 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−201·3	9·88	2·41	Condenser charged to 99·0 volts.
−195·8	1·95	2·66	Condenser charged to 17·8 volts.
−167·5	1·93	2·62	
−146·1	1·98	2·70	
−125·2	2·65	3·65	
−118·8	3·45	4·80	

IV. *Absolute Ethylic Alcohol* (C_2H_6O), having 10 per cent. of sodium dissolved in it.

Corrected galvanometer deflection when the condenser had air as dielectric = 4·04 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−193·0	12·95	3·11	Condenser charged to 99·7 volts.
−194·0	12·95	3·11	
−185·6	13·25	3·20	

V. *Acetone* (C_2H_6CO).

Corrected galvanometer deflection when the condenser had air as dielectric = 4·24 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−198·3	11·30	2·62	Condenser charged to 98·8 volts.
−195·2	2·05	2·49	Condenser charged to 18·7 volts.
−161·5	2·18	2·66	
−137·2	3·75	4·63	

VI. *Aniline* ($C_6H_5NH_2$).

Corrected galvanometer deflection when the condenser had air as dielectric = 4·05 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−191·2	12·2	2·92	Condenser charged to
−189·7	12·2	2·92	100 volts.
−145·5	12·55	3·00	
−114·7	12·9	3·09	
−80·0	14·78	3·55	

VII. *Carbolic Acid* (C_6H_6O).

Corrected galvanometer deflection when the condenser had air as dielectric = 4·34 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−200·0	11·25	2·54	Condenser charged to
−189·5	11·18	2·53	98·2 volts.
−166·2	11·25	2·33	
−156·5	11·15	2·52	
−137·3	11·50	2·60	

VIII. *Tetraethylammonium hydroxide* ($N(C_2H_5)_4HO$).
(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·24 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−200·5	13·63	3·15	Condenser charged to 98·7 volts.
−189·8	3·00	3·87	Condenser charged to
−165·7	5·75	7·52	17·8 volts.

IX. *Chinoline* (from Cinchonine).

Corrected galvanometer deflection when the condenser had air as dielectric = 4·05 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−195·0	11·1	2·67	Condenser charged to
−165·2	11·55	2·77	99·0 volts.
−106·0	8·50	11·5	Condenser charged to
−93·6	9·75	13·3	18·0 volts.

X. *Bisulphide of Carbon* (CS_2).

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−200·7	9·43	2·24	Condenser charged to
−183·8	9·60	2·27	98·2 volts.
−116·2	11·35	2·70	
−74·2	11·10	2·64	
+20·0	11·11	2·64	

XI. *Solution of Sulphur in Bisulphide of Carbon.*

Corrected galvanometer deflection when the condenser had air as dielectric = 4·04 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−152·8	11·0	2·65	Condenser charged
+20·0	11·5	2·77	to 99·2 volts.

XII. *Ethylic Ether* ($C_4H_{10}O$).

Corrected galvanometer deflection when the condenser had air as dielectric = 3·96 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−197·0	9·75	2·31	Condenser charged
−194·0	10·95	2·61	to 100 volts.
−180·2	9·52	2·25	
−168·8	10·25	2·43	
−161·2	15·35	3·69	

XIII. *Ethyl Nitrate* ($C_2H_5NO_3$).

Corrected galvanometer deflection when the condenser had air as dielectric = 4·34 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−203·8	12·25	2·73	Condenser charged to 99·8 volts.
−190·0	12·33	2·75	
−188·4	13·05	2·92	

XIV. *Castor Oil.*

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−197·7	9·30	2·19	Condenser charged to 98·2 volts.
−138·3	9·75	2·31	
+ 20·0	17·5	4·23	

XV. *Olive Oil.*

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in cm.	Dielectric constant.	Observations.
−147·2	9·2	2·18	Condenser charged to 98·2 volts.
+ 20·0	12·0	2·86	

The observations on castor oil, olive oil, and bisulphide of carbon are of considerable interest.

In the first place they show that the method we are using gives results at normal temperatures which are in agreement with those of other observers. Thus Dr. J. Hopkinson* found the dielectric constant K of carbon bisulphide at 20° C. to be 2·67: we find it to be 2·64. The square of the refractive index of carbon bisulphide for the D ray at 10° C. is 2·673, and accordingly this body is generally said to be one of those which obey Maxwell's law, in that its refractive index is nearly equal to the square root of the dielectric constant. If, however, we calculate the square of the refractive index of bisulphide of carbon for waves of infinite wave-length using at least

* See 'Roy. Soc. Proc.,' vol. 43, p. 161, 1887.

two terms of the Cauchy's formula,* we find that for carbon bisulphide the value of $(\mu_\infty)^2$ is 2·01, and this is somewhat less than the value (2·67) of K at ordinary temperatures. It is, however, much nearer to the value (2·24) which we find for K at the liquid air temperature. Again, Dr. J. Hopkinson's value for the dielectric constant of castor oil at normal temperature is 4·78, and the square of the refractive index of this body for infinite waves is 2·153. Our value for the dielectric constant of castor oil is 4·23 at 20° C. and 2·19 at −185° C.

Hence, in the case of castor oil at the temperature of liquid air there is practically a very fair agreement between the values of K and $(\mu_\infty)^2$ for a low frequency of electromotive force reversals, whereas at normal temperatures K has nearly twice the value of $(\mu_\infty)^2$.

The same fact holds good for olive oil. At normal temperatures its dielectric constant K is 3·16, and the square of its infinite refractive index $(\mu_\infty)^2$ is 2·131. At the temperature of liquid air we find the dielectric constant of olive oil to be 2·18 for low frequency. Hence, in these cases a large part of the difference between K and $(\mu_\infty)^2$ which exists at normal temperature disappears at very low temperatures, and castor oil and olive oil then make in this respect even a better agreement than carbon bisulphide. One other point is of interest. The temperature variation of dielectric constant for both olive oil and carbon bisulphide appear to be negative at ordinary temperatures,† that is to say, their dielectric constants decrease with rise of temperature. At low temperatures, however, we find their constants to have a positive temperature coefficient; in other words, they increase with temperature. Hence, it follows that, like ice and glycerine, they have at some temperature a *maximum* value for their dielectric constant.

The following table shows the relative values of the dielectric constants (K) of certain of these organic bodies at 15° and at −185° C for low frequency. At the same time the value of $(\mu_\infty)^2$ calculated by Cauchy's formula is placed against them as far as we have been able to find it determined.

It is clear, therefore, that in all the above cases a very low temperature extinguishes the high dielectric values many organic bodies have in the liquid condition, and it produces the same effect, therefore, as a sufficient increase in the frequency of the electromotive force reversals. Either of these operations tends to bring down the high value of the dielectric constant to a value much more comparable in magnitude with the square of the optical refractive index as calcu-

* We here make the assumption that even for a highly dispersive body like carbon bisulphide, the Cauchy formula is applicable for calculating $(\mu_\infty)^2$, an assumption perhaps not warranted.

† See Cassie, 'Roy. Soc. Proc.,' vol. 46, 1889.

Dielectric Constants of some Organic Bodies at Normal and at very Low Temperatures for Low Frequencies.

Substance.	Dielectric constant (K)		(Infinite Refractive Index)^2 = $(\mu_\infty)^2$.
	at 15° C.	at − 185° C. (Dewar and Fleming.)	
Methyl alcohol.....	34·0 (Thwing)...	3·13	—
Ethyl alcohol...	25·8 (Nernst)....	3·11	1·831
Amyl alcohol	16·0 (Nernst)....	2·14	1·951
Formic acid	62·0 (Thwing)...	2·41	—
Acetone	21·85 (Thwing)...	2·62	—
Ethylic ether	4·25 (Nernst)....	2·31	1·805
Castor oil.....	4·78 (Hopkinson)	2·19	2·153
Olive oil.....	3·16 „	2·18	2·131
Bisulphide of carbon	2·67 „	2·24	2·010
Aniline.....	7·51 (Smale)	2·92	—
Carbolic acid	.. ..	2·54	—
Ethyl nitrate.....	17·72 (Thwing)..	2·73	—

lated by Cauchy's formula for infinite wave-lengths, on the assumption that there is no anomalous dispersion.

The determination of the actual values of the refractive indices of these bodies at the liquid air temperature is a work of great difficulty, and one which we have not yet attempted. There is no question that it is important also to determine whether these very low temperatures have any effect in altering the absorptive power of these organic bodies for ultra-red rays, and removing that anomalous dispersion which appears to exist in some of them beyond the limits of the visible spectrum.

We are much indebted to Mr. J. E. Petavel for assistance in the work of taking the above described observations.

"On the Dielectric Constants of Metallic Oxides dissolved or suspended in Ice cooled to the Temperature of Liquid Air." By JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, and J. A. FLEMING, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London. Received June 29, 1897.

By the aid of the cone condenser and the method described by us in another communication,* we have studied the changes produced in the dielectric constants of ice at and above the temperature of liquid air when the ice has suspended in it insoluble metallic oxides or else has dissolved in it soluble metallic oxides or hydrates. We have also examined the dielectric properties of some of these oxides and hydrates when dissolved in other menstrua frozen and reduced to very low temperatures.

The experiments were conducted exactly as described in the communication above mentioned, and the frequency of the electromotive force reversals was, as before, 120.

In the following tables the four columns of figures give respectively the platinum temperature of the dielectric, the scale deflection of the galvanometer, representing the capacity of the condenser with the selected dielectric, when corrected for voltage and capacity of the leads and vibrator; the deduced dielectric constant, and the electromotive force at which the charging of the condenser was conducted.

The figure given at the head of each table as the *corrected galvanometer deflection with air as dielectric*, represents the capacity of the condenser with gaseous air at normal pressure and temperature as its dielectric.

The hydrates dissolved in water or other media, or the oxides in a very fine state of division, suspended in water, were introduced by means of a pipette, as the dielectric into the cavity of the cone condenser. This liquid dielectric was then frozen and reduced to the temperature of liquid air by lowering the whole condenser into a large vacuum vessel holding several litres of liquid air. As soon as the dielectric was reduced to the temperature of $-188°$ C. the observations for capacity and temperature began.

In all cases an observation was taken for electric conductivity and the dielectric measurements were stopped the moment any sensible conductivity made its appearance.

* See Fleming and Dewar, "On the Dielectric Constants of Certain Frozen Electrolytes at and above the Temperature of Liquid Air," 'Roy. Soc. Proc.,' vol. 61, p. 299.

The first experiments were conducted with solutions of various alkaline hydrates taking, to begin with, 5 per cent. aqueous solutions of the hydrates of lithium, sodium, potassium and rubidium. Following on this, an experiment was tried with a 2·85 per cent. aqueous solution of cæsium hydrate. The details of the observations and results are given in Tables I, II, III, IV, and V.

I. *Dielectric Constant of Lithium Hydrate* (LHO).

(5 per cent. solution in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 3·03 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−202·8	9·30	3·00	Condenser charged to 99 volts.
−198·0	2·00	3·23	Condenser charged to 19·8 volts.
−180·6	2·20	3·57	
−144·7	3·20	5·23	
−133·8	4·10	6·74	
−118·2	9·85	16·20 ?	

II. *Dielectric Constant of Sodium Hydrate* (NaHO).

(5 per cent. solution in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·02 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric. constant.	Observations.
−201·8	7·70	133·0	Condenser charged to 1·434 volts.
−201·7	7·72	134·0	
−183·0	8·40	145·0	
−174·2	8·27	143·0	
−168·2	8·15	141·0	
−163·8	8·15	141·0	

III. *Dielectric Constant of Potassium Hydrate* (KHO) (*Caustic Potash*).

(5 per cent. solution in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 3·91 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−204·8	6·92	123·0	Condenser charged with 1·434
−200·7	7·07	126·0	volts.
−196·2	7·35	131·0	
−181·5	7·55	135·0	
−170·1	7·49	133·0	
−158·7	7·33	131·0	
−147·3	7·40	132·0	
−135·9	7·54	134·0	
−127·7	8·01	143·0	

IV. *Dielectric Constant of Rubidium Hydrate* (RbHO).

(5 per cent. solution in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 3·03 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·3	3·50	81·6	Condenser charged to 1·434 volts.
−201·5	3·57	82·3	
−201·5	3·47	80·0	
−198·7	5·65	130·0	
−197·8	6·45	148·0	
−195·8	7·37	169·0	
−183·2	7·50	172·0 ?	
−175·1	8·70	200·0	
−169·8	8·85	202·0	
−146·1	8·97	206·0	
−138·8	9·10	209·0	

V. *Dielectric Constant of Cæsium Hydrate* (CsHO).

(2·85 per cent. solution in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−203·5	11·92	2·76	Condenser charged to 100·3 volts.
−178·0	2·40	2·79	Condenser charged to 20·2 volts.
−143·0	3·05	3·57	

In order to examine again the influence of the solvent, we tried a saturated solution of caustic potash (KHO) in ethylic alcohol. We have already shown that a 5 per cent. aqueous solution of caustic potash has an enormously greater dielectric constant at the temperature of liquid air than the alcoholic 5 per cent. solution. And we now find that the saturated alcoholic solution of caustic potash has a small dielectric value, as shown in Table VI.

VI. *Dielectric Constant of Caustic Potash* (KHO) *dissolved in Alcohol*

(Saturated solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·24 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−194·8	14·55	3·40	Condenser charged to 98·2 volts.
−187·0	14·85	3·46	
−171·7	15·25	3·57	
−159·0	15·00	3·50	
−146·2	25·70	5·97 ?	Charged to 18 volts.

We next selected for trial thallium hydrate dissolved in water, with the results shown in Table VII.

VII. *Dielectric Constant of Thallium Hydrate* (TlHO).

(5·74 per cent. solution in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·04 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−203·0	4·82	6·54	Condenser charged to 18 volts.
−192·0	10·50	1·43	
−173·0	2·40	41·4	Condenser charged to 1·43 volts.
−164·3	4·10	70·7	
−148·5	6·60	114·0	
−123·0	6·85	118·0	
−112·8	7·50	130·0	
−105·8	8·30	143·0	

Our next experiment was made with barium hydrate, taking a 5 per cent. mixture suspended in water.

VIII. *Dielectric Constant of Barium Hydrate* ($Ba(HO)_2$).

(5 per cent. suspended in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 3·03 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·3	9·00	15·1	Condenser charged to 19·5 volts.
−199·5	10·35	17·2	
−196·8	11·90	20·1	
−186·8	12·55	21·2	
−178·0	14·20	23·9	
−174·2	1·11	25·5	Condenser charged to 1·434 volts.
−161·0	1·40	32·1	
−147·2	1·60	36·7	
−139·0	1·71	39·3	
−133·7	1·82	41·7	
−124·2	1·95	44·8	
−121·0	2·17	51·8 ?	

Following on these experiments we commenced a series of measurements on the dielectric constant of ice having suspended in it various insoluble oxides. These oxides, freshly precipitated and

washed, were suspended in distilled water, and, being well shaken up, were introduced into the condenser and frozen. The results are given in the tables below, beginning with aluminic oxide.

IX. *Dielectric Constant of Aluminic Sesquioxide* (Al_2O_3).

(Suspended in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 3·97 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−205·7	1·92	2·58	Condenser charged to 18·0 volts.
−190·0	1·89	2·53	
−159·8	1·92	2·58	
−132·7	3·25	4·43	

X. *Dielectric Constant of Oxide of Iron* (Fe_2O_3).

(Suspended in water.)

(12·6 per cent. of oxide.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·07 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−199·4	5·2	89·8	Condenser charged to 1·434 volts.
−191·7	6·3	112·0	
−180·0	6·8	121·0	
−165·4	7·0	124·0	
−153·8	7·2	128·0	
−143·7	7·75	134·0	
−136·8	7·8	135·0	
−128·7	8·05	139·0	
−123·2	8·75	151·0?	
−118·0	10·3	176·0?	

XI. *Dielectric Constant of Oxide of Copper* (CuO).

(Suspended in water.)

(14·55 per cent. of oxide.)

Corrected galvanometer reading when the condenser has air as dielectric = 4·07 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−195·7	9·40	161	Condenser charged to 1·434 volts.
−185·0	9·25	158	
−174·8	9·52	163	
−164·3	9·60	164	
−159·0	9·50	162	
−151·2	9·60	164	
−141·3	9·90	169	
−130·0	10·30	176 ?	
−124 2	11·10	190 ?	
−120·5	12·15	207 ?	

It will be noticed that the dielectric constant of the ice having in suspension particles of oxide of copper has an exceedingly high value, even at the temperature of liquid air, and, moreover, shows no signs of becoming reduced. We suspected that this might be due to the conductivity of the oxide of copper particles, which, though separated and insulated by ice at a low temperature in such fashion that the whole mass is a good insulator, may yet raise the value of the average dielectric constant.

We tried suspending sulphide of copper in ice, but in this case found the material, as a whole, had such conductivity that we could not make any dielectric measurement by the method used. With this exception, all the other frozen oxides and hydrates suspended or dissolved in ice had a very high resistance.

We then tried dissolving the oxide of copper in ammonia.

XII. *Dielectric Constant of Oxide of Copper* (CuO).

(Dissolved in ammonium hydrate.)

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·15 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·3	10·2	2·39	Condenser charged to 98·7 volts.
−178·0	2·0	2·34	Condenser charged to 19·8 volts.
−132·2	2·35	2·77	

XIII. *Dielectric Constant of Oxide of Lead* (PbO).

(Suspended in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·04 for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−197·4	4·56	78·5	Condenser charged to 1·434 volts.
−184·8	5·23	90·0	
−159·2	5·40	93·0	
−150·2	5·40	93·0	
−128·3	5·40	93·0	

In contrast with the above results, we tried dissolving the oxide of lead in caustic potash, and prepared a saturated solution of oxide of lead (PbO), dissolved in a 5 per cent. solution of caustic potash.

XIV. *Dielectric Constant of Oxide of Lead* (PbO).

(Dissolved in a 5 per cent. solution of caustic potash.)

Corrected galvanometer deflection with air as dielectric = 3·03 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−189·2	7·47	171	Condenser charged to 1·434 volts.
−170·2	7·45	171	
−158·0	7·52	173	
−148·4	7·60	175	
−138·2	7·45	171 ?	
−127·8	7·77	179	

Finally we tried bismuth oxide suspended in water, with the results shown in Table XV.

XV. *Dielectric Constant of Bismuth Oxide* (Bi_2O_3).

(Suspended in water.)

Corrected galvanometer deflection when the condenser has air as dielectric = 3·97 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−200·0	1·97	2·64	Condenser charged to 17·8 volts.
−199·7	1·93	2·62	
−194·0	2·00	2·72	
−185·2	2·00	2·72	
−171·0	2·00	2·72	
−159·0	2·10	2·88	
−152·5	2·45	3·46	
−142·5	4·40	6·13	
−138·0	6·15	8·55	
−134·4	9·90	13·80	
−129·2	14·15	19·9	
−127·3	1·41	24·5	Condenser charged to 1·434 volts.
−122·7	2·15	37·8	
−117·5	3·25	57·2	
−111·0	4·45	78·2	
−107·3	5·10	90·2	

Before drawing conclusions as to the dielectric power of these oxides and hydrates, it seemed important to try the effect of suspending in ice small quantities of inert particles, and we therefore selected *finely divided gold* and *precipitated sulphur* as instances of a good conductor and a good non-conductor in very fine states of division. These finely divided materials were suspended in water and frozen and the dielectric constants taken as follows :—

Dielectric Constant of Ice, having in suspension a small quantity of finely divided Gold.

Corrected galvanometer deflection when the condenser has air as dielectric = 4·04 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−199·8	10·30	2·60	Condenser charged to 98·5 volts.
−196·0	9·90	2·39	
−161·7	5·25	7·12	Condenser charged to 18·0 volts.
−149·0	11·0	15·0	
−144·5	17·0	23·2	

Dielectric Constant of Ice, having in suspension a small quantity of finely divided Sulphur.

Corrected galvanometer deflection when the condenser has air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−198·2	11·1	2·64	Condenser charged to 98·2 volts.
−182·5	11·3	2·69	
−160·0	4·2	5·44	Condenser charged to 1·43 volts.

The results show that practically no influence is produced upon the dielectric quality of the ice at very low temperatures when it is impregnated either with conducting particles of gold or non-conducting particles of sulphur. The amount of gold in the ice as used by us is no doubt small. In each case the dielectric constant of the material is very close to that of the ice obtained from pure distilled water.

The electrical resistance of the ice slightly impregnated with finely divided gold did not appear to be less than that of ordinary ice. To the tests we applied the one appeared to be as good an insulator as the other.

The foregoing observations are graphically delineated in the diagrams in figs. 1, 2, and 3, according to the magnitude of the dielectric constants found. The following facts may then be noted:—

(i) The presence of cæsium hydrate, lithium hydrate, bismuth oxide, aluminic oxide, finely divided sulphur or finely divided gold, in the percentages used by us, in ice makes little or no difference in its dielectric power when cooled to the temperature of liquid air.

(ii) The presence to about the same extent of the hydrates of potassium, sodium, rubidium, or the oxides of copper, iron, or lead, causes a very great increase in the dielectric power of the ice at that low temperature.

(iii) The dielectric curves are, however, all seen to be tending downwards in such a way as to show that at still lower temperatures it is probable the effect of the oxides or hydrates would be annulled and the dielectric constants of all be reduced to a value not far from that of pure ice.

(iv) The exception to this tendency, so far as yet observed, appears to be in the case of the oxide of copper, which, when suspended in ice, causes it to possess the remarkably high dielectric value of 150 even at the temperature of liquid air.

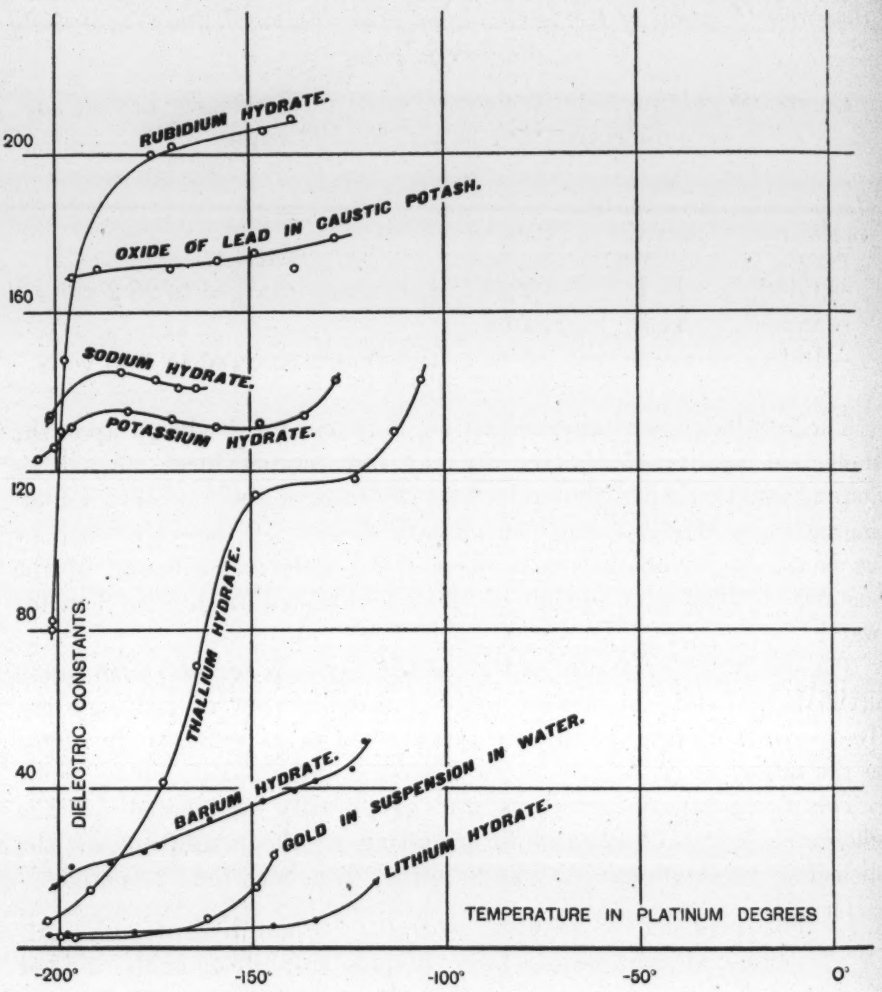


FIG. 1.—Curves showing the variation in Dielectric Constant with Temperature of various Oxides and Hydrates.

It is possible, however, that in this case the conductivity of the yet insulated particles of oxide may account for the result.

(v) The effect of the solvent or menstruum upon the dielectric constant is also exceedingly marked. Thus the oxide of copper suspended in ice has a dielectric constant of about 150 at −185°, the oxide of lead suspended in ice has a value of about 80 at the same temperature. If, however, the oxide of copper is dissolved in ammonia and frozen, the dielectric constant of this at −185° falls to 2·2, whereas if the oxide of lead is dissolved in 5 per cent. solution of caustic potash and frozen, the dielectric constant rises to 170 at the temperature of liquid air.

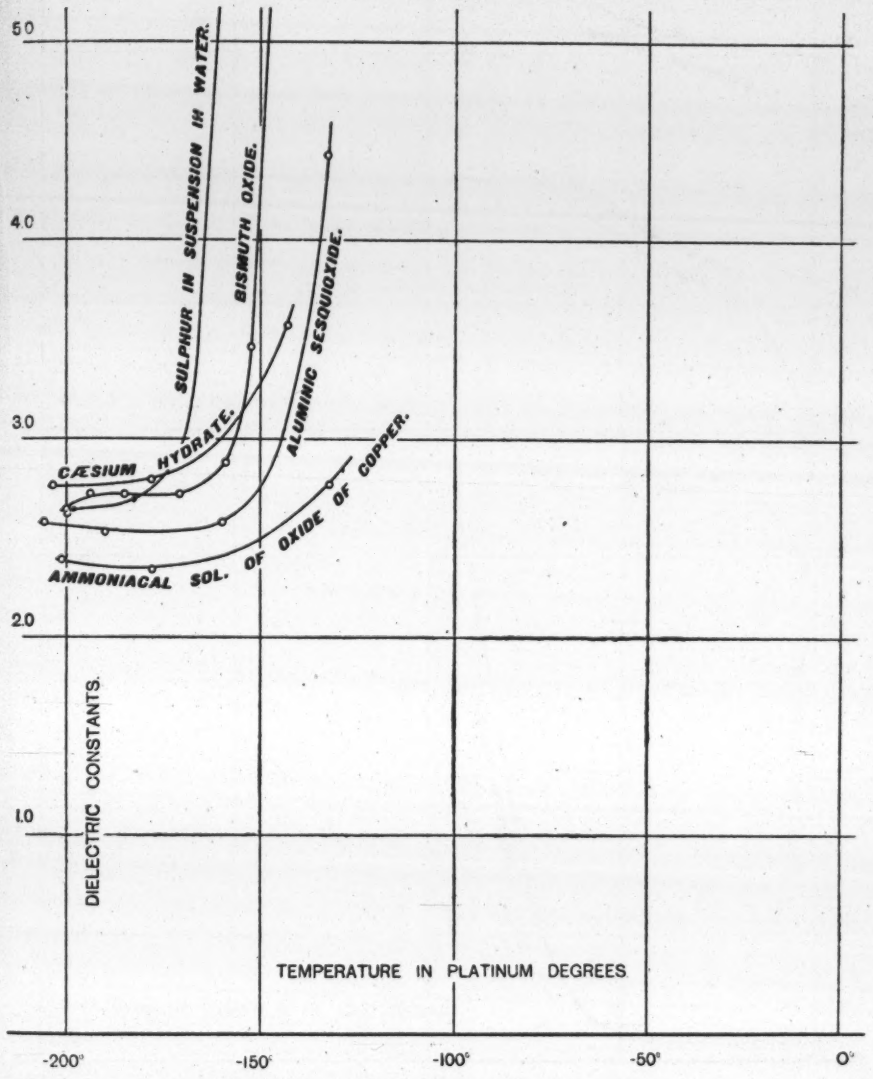


FIG. 2.—Curves showing the variation in Dielectric Constant with Temperature of various Oxides and Hydrates.

The only general conclusion that can as yet be drawn from the experiments described is that the presence of certain oxides or hydrates of metals in ice, even to the extent of not many per cent., has an immense influence in raising the dielectric power of the ice when taken at the temperature of liquid air, whereas the presence of other oxides or hydrates chemically analogous has hardly any influence at all.

In the above described experiments, Mr. J. E. Petavel has given us considerable assistance.

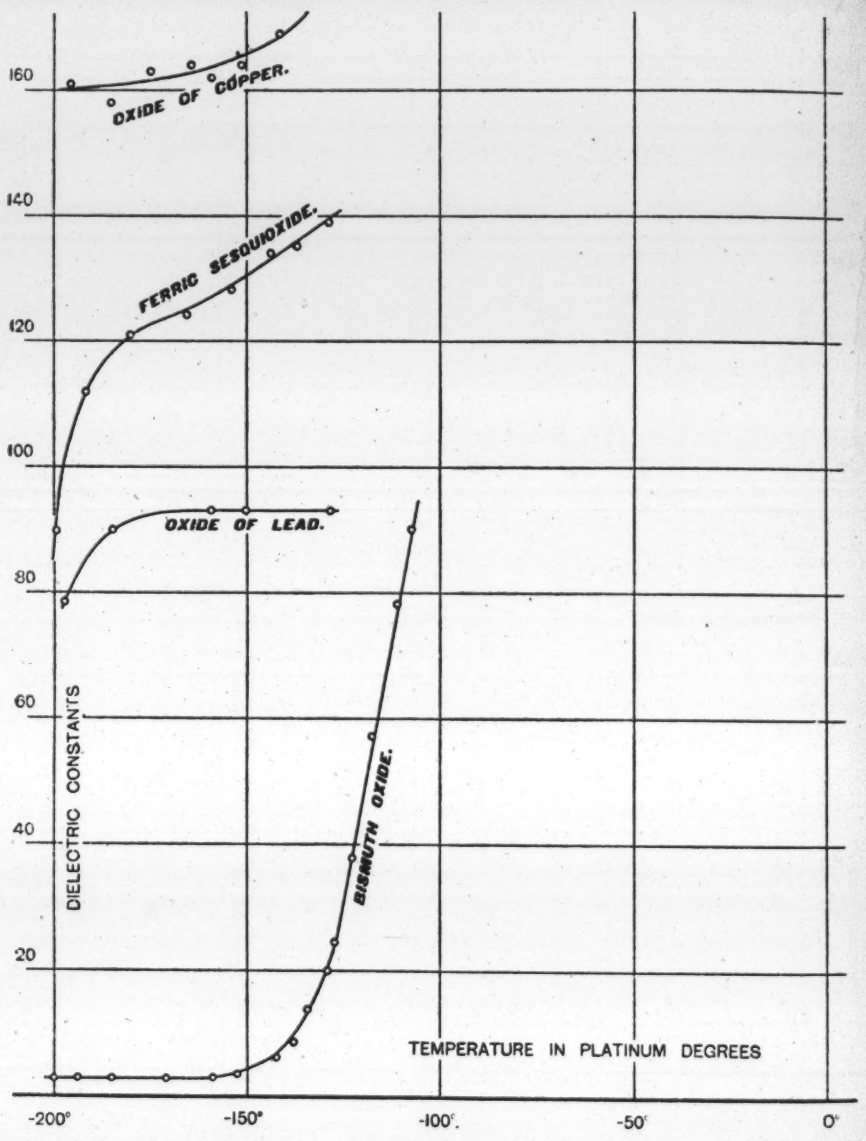


Fig. 3.—Curves showing the variation in Dielectric Constant with Temperature of various Oxides.

"Further Observations on the Dielectric Constants of Frozen Electrolytes at and above the Temperature of Liquid Air." By J. A. Fleming, M.A., D.Sc., F.R.S., Professor of Electrical Engineering in University College, London, and James Dewar, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution. Received June 29, 1897.

In continuation of our examination of the dielectric constants of frozen electrolytes at very low temperatures, we have subjected to

test (in the manner described in a former abstract)* another large series of electrolytes.

Referring to the above-mentioned abstract for an account of the details of the method, we merely give in the following Tables the results of our observations on the dielectric constants of many aqueous solutions of metallic salts, which have been taken with the object of determining if possible the causes which create high or low dielectric values at low temperatures.

The dielectric observations were all taken at a frequency of electromotive force reversal of 120.

We have taken, in the first place, a large series of observations on electrolytes with the same base (sodium), but different acid radicles, and then a number with the same acid radicle but different bases. Also, we have examined normal and corresponding acid salts, double salts, and many salts taken in such proportion with water as to yield the so-called cryohydrates.

The following tables give the details of the dielectric measurements.

I. *Dielectric Constant of Sodic Acetate* ($NaC_2H_3O_2$).

(5 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·02 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−200·0	3·00	3·82	Condenser charged to 19·0 volts.
−200·2	3·03	3·85	
−197·6	3·20	4·08	
−181·5	5·20	6·70	
−158·5	1·72	29·80	Condenser charged to 1·434 volts.
−150·8	2·45	42·50	
−142·8	3·05	52·80	
−113·3	4·17	72·00	
−98·8	5·15	89·0 ?	

* See Fleming and Dewar, "On the Dielectric Constants of Certain Frozen Electrolytes at and above the Temperature of Liquid Air." 'Roy. Soc. Proc.,' vol. 61, p. 299.

II. *Dielectric Constant of Normal Sodic Carbonate* (Na_2CO_3).

(6 per cent. (cryohydrate) solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·24 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−198·5	2·10	34·4	Condenser charged to 1·434 volts.
−192·2	2·35	38·4	
−171·0	2·55	41·7	
−159·0	2·60	42·7	
−144·2	2·82	46·2	
−134·8	3·40	55·7	
−122·0	4·70	77·0	
−114·3	5·50	90·2	
−104·0	7·15	118·0	
−94·0	8·90	146·0	

N.B. The condenser was warmed up in the vacuum tube.

III. *Dielectric Constant of Sodic Bicarbonate* ($NaHCO_3$).

(6 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·34 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−195·3	12·20	2·74	Condenser charged to 99·0 volts.
−183·3	3·70	4·24	Condenser charged to 19·8 volts.
−166·7	3·00	48·70	
−113·0	4·40	70·70	Condenser charged to 1·434 volts.
−90·5	4·65	74·70	

N.B.—The condenser was warmed up out of the vacuum tube.

IV. *Dielectric Constant of Sodic Biborate* (*Borax*), $Na_2O2B_2O_3,10H_2O$.

(15 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·24 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−202·0	2·40	39·4	Condenser charged to 1·434 volts.
−195·3	4·10	67·2	
−188·4	4·85	79·5	
−176·0	5·25	86·3	
−152·7	5·50	90·5	
−139·8	5·85	96·0	
−127·0	6·40	105·0	
−121·0	6·30	103·0	
−112·8	7·35	118·0	

N.B.—Condenser warmed up in the vacuum tube.

V. *Dielectric Constant of Sodic Nitrite* ($NaNO_2$).

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·15 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−200·0	11·80	2·78	Condenser charged to 98·8 volts.
−172·7	2·35	2·77	Condenser charged to 19·8 volts.
−130·3	13·50	16·30	Condenser charged to 19·8 volts.
−120·3	2·35	39·00	Condenser charged to 1·434 volts.
−98·2	3·40	57·00	Condenser charged to 1·434 volts.

VI. *Dielectric Constant of Sodic Hyposulphite* ($Na_2S_2H_2O_4,4H_2O$).

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·15 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−200·8	4·40	74	Condenser charged to 1·434 volts.
−183·0	4·75	80	
−163·7	5·05	85	
−143·8	5·75	96	
−130·7	7·02	118	
−122·7	7·20	121	
−114·8	7·67	129	
−107·7	8·65	145	
−98·4	11·15	188 ?	

VII. *Dielectric Constant of Sodic Silicate.*

(5 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·15 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−200·0	3·42	57·4	Condenser charged to 1·434 volts.
−195·5	6·15	103·0	
−175·0	7·25	122·0	
−164·7	7·45	125·0	
−148·2	7·60	128·0	
−138·8	7·60	128·0	
−129·7	7·80	131·0	
−106·2	8·15	137·0	
−96·0	8·65	145·0	

VIII. *Dielectric Constant of Sodic Chloride* (NaCl).

(23·6 per cent. (cryohodrate) solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 3·44 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Charging volts.	Observations.
−203·1	9·77	2·78	99·20	90,000 ohms in series for the first two readings, then 1000 ohms.
−200·0	9·74	2·62	92·20	
−192·0	10·20	2·90	99·20	
−177·0	12·85	3·68	99·20	
−165·2	16·45	4·73	99·20	The condenser was warmed up out of the vacuum tube.
−156·5	4·05	5·88	19·80	
−122·7	18·60	27·40	19·80	
−108·3	2·25	156·00 ?	1·43	

IX. *Dielectric Constant of Sodic Chlorate* ($NaClO_4$).

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−202·4	5·65	6·68	Condenser charged to 20·2 volts.
−189·8	6·10	7·22	
−173·7	6·35	7·51	
−156·0	8·05	9·57	
−114·3	3·55	59·20	Condenser charged to 1·434 volts.
−98·0	5·45	91·20	

X. *Dielectric Constant of Normal Sodic Sulphate* (Na_2SO_4).

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−197·0	4·75	5·68	Condenser charged to 20·0 volts.
−167·3	5·05	6·04	
−129·8	7·15	8·58	
−116·6	8·35	10·00	
−85·8	8·95	10·70	
−69·0	9·65	11·60	

XI. *Dielectric Constant of Sodic Bisulphate* ($NaHSO_4$).

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·8	12·1	2·83	Condenser charged to 100·2 volts.
−191·3	5·5	6·82	Condenser charged to 19·3 volts.
−158·7	12·5	15·60	

XII. *Dielectric Constant of Hydro-disodic Phosphate* (Na_2HPO_4).

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−199·2	1·83	31·1	Condenser charged to 1·4 volts;
−180·0	1·95	33·0	no resistance in series with
−156·0	2·10	35·4	galvanometer.
−141·3	2·45	41·5	
−133·2	2·97	50·2	
−123·0	3·75	53·5	
−102·2	5·45	92·2	

XIII. *Dielectric Constant of Potassium Ferrocyanide.*

(12 per cent. (cryohydrate) solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·02 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−204·3	2·02	35·1	Condenser charged to 1·434 volts.
−203·8	2·01	34·9	
−200·0	2·10	36·4	
−187·2	2·30	39·8	
−168·1	2·46	42·4	
−156·5	2·56	44·3	
−145·8	2·87	49·8	
−129·5	3·95	68·5	
−119·4	4·70	81·7	
−111·4	5·60	97·0	
−107·0	6·65	115·0	
−105·3	7·25	126·0	
−102·0	8·50	147·0	
−98·8	9·60	168·0 ?	

XIV. *Dielectric Constant of Potassium Chromate* ($KCrO_4$).

(36 per cent. (cryohydrate) solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·34 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·5	8·95	10·2	Condenser charged to 20·0 volts.
−193·7	9·75	11·1	
−177·0	10·62	12·1	
−165·0	10·95	12·5	
−148·8	11·05	12·6 ?	
−135·7	12·20	14·0	
−127·0	12·75	14·6	
−122·0	13·40	15·3	
−112·4	14·65	16·9	
−100·0	1·39	22·3	Condenser charged to 1·434 volts.

XV. *Dielectric Constant of Potassium Bichromate* ($K_2Cr_2O_7$).

(5·3 per cent. (cryohydrate) solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·15 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−173·0	11·15	2·50	Condenser charged to 99 volts.

N.B.—The supply of liquid air on the occasion of this measurement was not sufficient to lower the temperature any further than −173°, but it is clear that the dielectric constant has a low value.

XVI. *Dielectric Constant of Potassium Bicarbonate* ($KHCO_3$).

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·34 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−196·3	1·90	2·07	Condenser charged to 19·8 volts.
−182·0	1·93	2·11	
−166·5	2 50	2·80	
−147·2	13·00	14·70	
−134·7	1·75	27·90	Condenser charged to 1·43 volts.
−117·8	2·60	41·70	

XVII. *Dielectric Constant of Potassic Iodide* (KI).

(52·17 per cent. (cryohydrate) solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 3·44 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−199·0	9·65	2·72	Condenser charged with 100 volts.
−196·0	9·90	2·80	1000 ohms in series with galvano-
−196·7	10·17	2·87	meter.
−191·6	12·85	3·04	Charging volts = 99·8.
−170·6	5·30	7·67	Charging volts = 19·8.
−148·0	14·75	21·60	Charging volts = 19·8.

XVIII. *Dielectric Constant of Potassium Alum.*

(9 per cent. solution.)

Corrected galvanometer deflection when condenser had air as dielectric = 3·12 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−192·2	12·35	3·86	
−186·8	17·50	5·52	
−181·8	4·25	7·00	
−169·5	6·15	10·20	
−161·7	7·80	12·90	
−151·7	10·40	17·60	
−142·8	13·10	21·70	
−132·7	16·30	27·10	
−130·3	1·25	27·80	
−123·5	1·62	36·00	
−105·7	2·90	64·70 ?	
−66·8	3·23	72·20 ?	

XIX. *Dielectric Constant of Hydropotassic Sulphide* (KHS).

(5 per cent. solution.)

Corrected galvanometer deflection when condenser had air as dielectric = 3·44 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−199·8	10·14	2·86	Condenser charged with 100 volts.
−199·9	10·15	2·86	1,000 ohms in series with galvanometer.
−197·7	10·15	2·86	
−197·2	10·47	2·95	

XX. *Dielectric Constant of Molybdate of Ammonia.*

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·15 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−202·0	12·55	2·94	Condenser charged to 98·7 volts.
−150·2	4·95	82·80	Condenser charged to 1·434 volts.
−119·4	5·50	90·40	
−88·5	5·80	96·40	

XXI. *Dielectric Constant of Barium Chloride* ($BaCl_2$).

(23 per cent. (cryohydrate) solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·15 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−205·0	11·05	2·62	Condenser charged to 98·8 volts.
−198·2	2·15	2·52	Condenser charged to 19·8 volts.
−173·7	2·25	2·64	
−145·0	6·80	8·20	
−111·0	2·10	35·20	Condenser charged to 1·434 volts.
−101·7	4·05	69·00	
−84·8	9·75	164·20 ?	

XXII. *Dielectric Constant of Cupric Carbonate* ($CuCO_3$).

(10 per cent. in suspension in water.)

Corrected galvanometer deflection when the condenser had air as dielectric = 3·97 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−201·6	10·15	2·48	Condenser charged at 99·0 volts.
−202·0	10·10	2·47	
−198·0	10·10	2·47	
−189·8	10·08	2·46	
−178·3	10·08	2·46	
−168·3	10·15	2·48	
−158·0	10·23	2·50	
−149·2	10·55	2·58	
−146·7	11·40	2·80	
−142·0	2·25	3·02	Condenser charged at 18·2 volts.
−132·7	2·55	3·42	
−124·7	12·00	16·50	
−120·8	1·25	21·80	Condenser charged at 1·434 volts.
−112·9	1·90	33·30	
−104·3	2·70	47·00	
−85·0	3·00	52·60	
−78·5	2·90	50·80	
−69·0	3·20	56·20	

XXIII. *Dielectric Constant of Plumbic Nitrate*, $Pb(NO_3)_2$.

(10 per cent. solution.)

Corrected galvanometer deflection when the condenser had air as dielectric = 4·24 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−200·2	13·12	3·03	Charged to 98·7 volts.
−144·4	2·75	45·20	Charged to 1·434 volts.
−132·0	5·10	83·70	

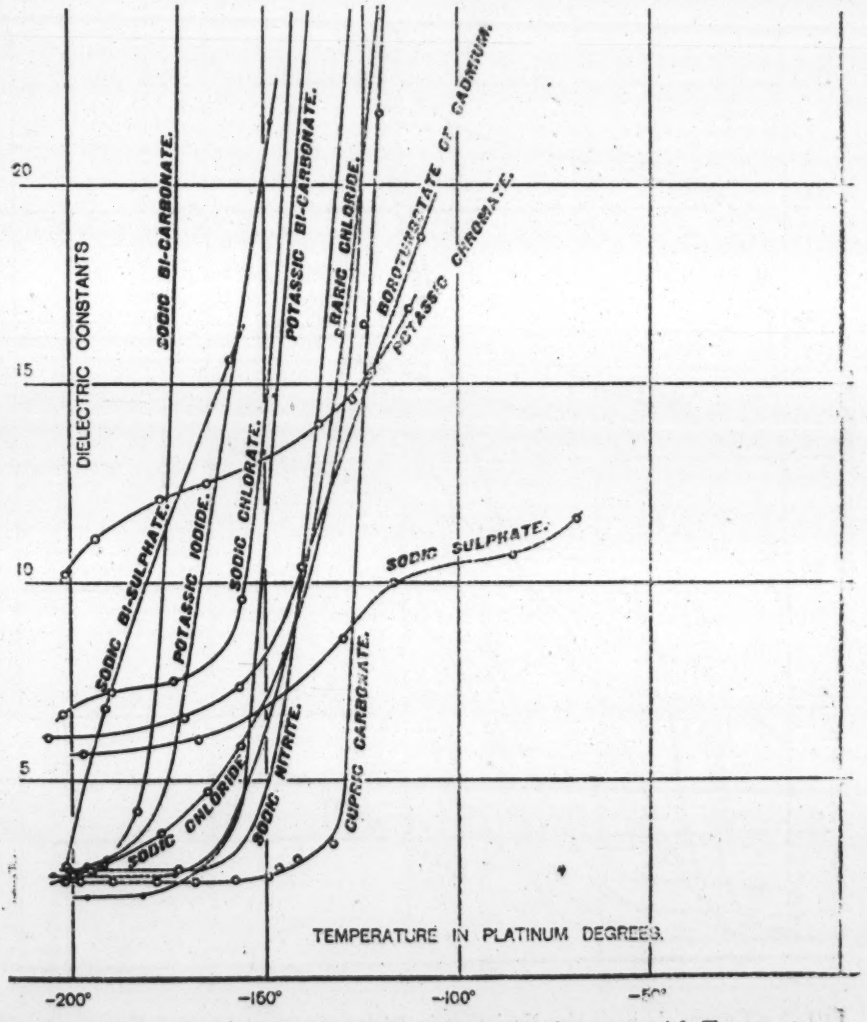


FIG. 1.—Curves showing the Variation in Dielectric Constant with Temperature of various Frozen Electrolytes.

XXIV. *Dielectric Constant of Borotungstate of Cadmium.*

Corrected galvanometer deflection when the condenser had air as dielectric = 4·13 cm. for 100 volts.

Temperature in platinum degrees.	Mean galvanometer deflection in centimetres.	Dielectric constant.	Observations.
−206·0	5·07	6·07	
−171·0	5·50	6·58	
−156·5	6·15	7·37	
−140·5	8·70	10·40	
−122·7	12·65	15·20	
−109·2	15·50	18·70	

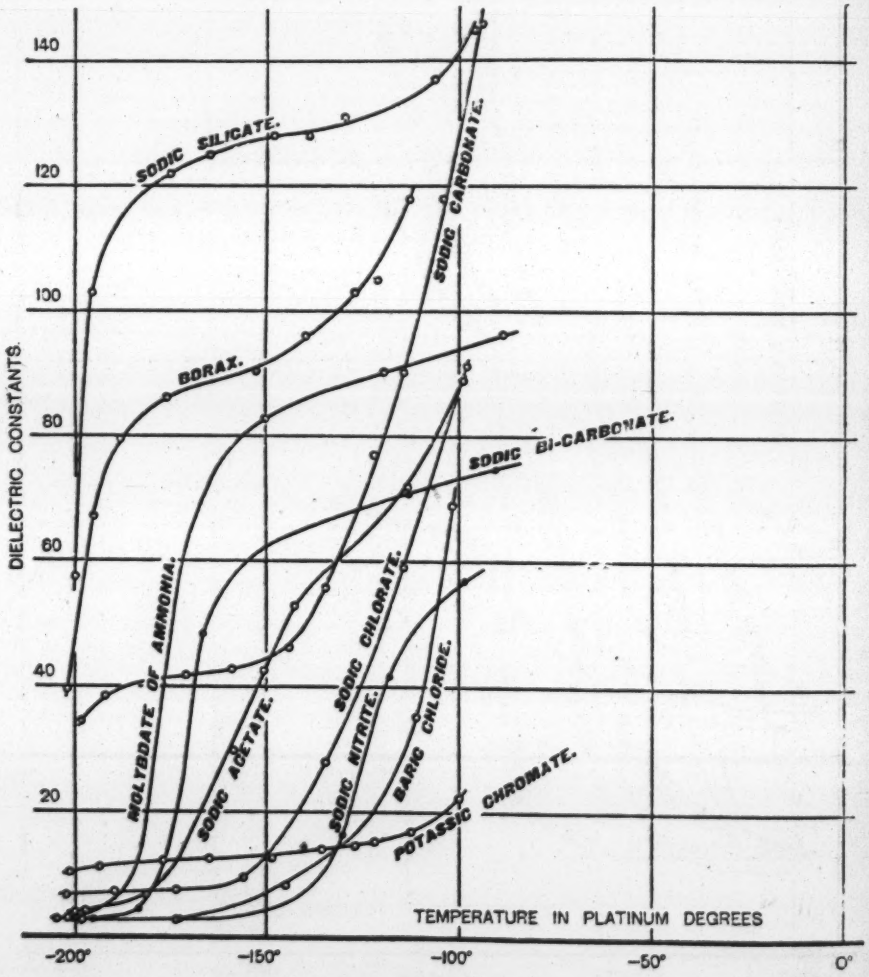


FIG. 2.—Curves showing the Variation in Dielectric Constant with Temperature of various Frozen Electrolytes.

The whole of the observations in the foregoing tables are delineated graphically in the charts in figs. 1, 2, and 3.

In the case of each dielectric measurement a preliminary resistance measurement was generally made to determine the electrical resistance of the frozen electrolyte. It was invariably found that in the case of all the true electrolytic solutions, the electrical resistance of the dielectric at the temperature of liquid air was exceedingly large, generally exceeding many thousands of megohms.

As the temperature of the dielectric rose the electrical resistance fell down, often very quickly, to a fraction of a megohm, and, long before the melting point of the electrolyte was reached, quite considerable conductive power always made its appearance in the frozen electrolytic solution.

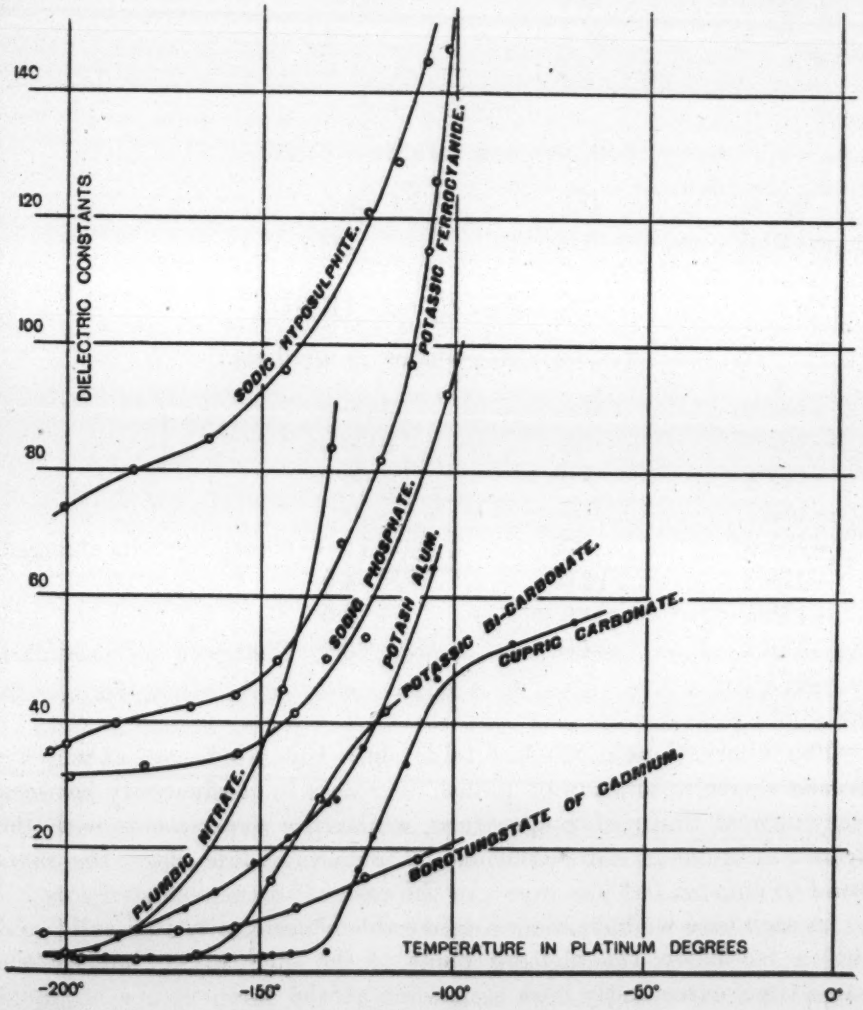


FIG. 3.—Curves showing the Variation in Dielectric Constant with Temperature of various Frozen Electrolytes.

The following tables illustrate the immense change in conductivity taking place in one or two cases within a range of from 10° to 100° :—

Resistance Measurements of Dielectrics at Low Temperatures.

Temperature of condenser in platinum degrees.	Galvanometer deflection in centimetres.	Approximate resistance of the condenser dielectric in megohms.	
		Sodic Chloride	
		(23·6 per cent. solution in water).	
−203·0	0·2	2500·0	
−125·0	0·4	1250·0	
− 90·5	26·0	02	
		Potassium Iodide	
		(52 per cent. solution in water).	
−200·0	0·1	5000·0	
−120·0	4·0	1·0	
		Potassium Hydrate (KHO)	
		(5 per cent. solution in alcohol).	
−187·0	0·1	5000·0	
−185·7	0·5	1000·0	
−183·9	5·0	100·0	
−182·1	10·0	50·0	
−181·5	4·5	11·0	Charging volts changed.
−179·1	12·0	4·0	
−178·5	20·0	3·0	
−176·1	4·0	0·9	Charging volts reduced.
−175·2	7·0	0·5	

The observations in the last table show the rapid rate at which a frozen alcoholic solution of potash increases in conductivity between very narrow limits of temperature, whilst the experiments with the frozen solutions of sodic chloride and potassic iodide show the same kind of change, but less rapid, in the case of aqueous electrolytes.

In each case we have very considerable conductivity in a solid condition far below the melting point of the electrolyte, but, at the same time, exceedingly high resistance at the temperature of liquid air.

On looking at the chart of curves it will be seen that the general

form of the dielectric temperature curve is not unlike the magnetisation curve of a ferro-magnetic body or a vapour tension curve.

In most of the curves there is within a certain range of temperature a fall more or less sudden *from* a high value of the dielectric constant *to* a low value.

As regards the value of the dielectric constants of frozen electrolytes at the temperature of liquid air, the salts employed by us may be divided into three broad classes:—

I. Those which, when added to water in percentages from 5 to 50, *do not much affect the dielectric constant* of the water when it is frozen; and which, at the temperature of liquid air, have dielectric constants not far from 2·5, or lying between 2 and 3. Such salts are *sodic bicarbonate, sodic bisulphate, potassic bichromate, potassic bicarbonate, sodic chloride, baric chloride, potassic iodide, sodic nitrite, hydropotassic sulphide, cupric carbonate.* These include the acid salts and halogen salts.

II. Those salts which, when added to water in percentages from 5 to 50, *raise the dielectric constant of the water* somewhat and which yield frozen electrolytes, having, at the temperature of liquid air, dielectric constants lying between 3 and 10, that of pure ice at the same temperature being 2·5. Such salts are *potassic chromate, sodic sulphate, sodic chlorate, cadmic borotungstate, sodic acetate, potassium aluminic sulphate, plumbic nitrate.* These are all highly oxygenated salts.

III. Salts which, when added to water in percentages from 5 to 50, yield electrolytes which, if frozen, have *immensely greater dielectric constants*, than pure ice at the temperature of liquid air, viz., values from 30 to 70.

Such salts are *sodic carbonate, sodic biborate, sodic hyposulphite, sodic silicate, hydrodisodic phosphate, potassium ferrocyanide.*

It will be noticed that whenever we have tested a normal salt and an acid salt of the same base, such as a carbonate and a bicarbonate, a chromate and a bichromate, a sulphate and a bisulphate, the acid salt always has the lower dielectric constant of the two at the temperature of liquid air.

With the exception of sodium carbonate, potassium ferrocyanide, and hydrodisodic phosphate, the whole of the dielectric curves seem to be tending downwards in such a way as to show that at somewhat lower temperatures than we have at command, the whole of these frozen electrolytes would have dielectric constants not far from that of pure ice. In other words, would be reduced to values probably near 2 or 3.

There seems now a fair amount of evidence to show that with a

few exceptions all frozen electrolytes would, in all probability, if reduced to temperatures not far above the absolute zero, have their dielectric constants approximately equal, and reduce to a value not far from 2 or 3. At the same time the electric resistivity of such frozen electrolytes would tend to become infinite as the temperature is continuously reduced.

In the very great labour of taking and reducing the above numerous observations, Mr. J. E. Petavel has rendered us much valuable service.

June 17, 1897.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

Sir W. H. Broadbent, Mr. Charles Chree, Mr. H. J. Elwes, Professor G. B. Howes, Mr. F. S. Kipping, Professor G. B. Mathews, Mr. F. H. Neville, Professor J. M. Thomson, and Professor F. T. Trouton were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "An Experimental Research upon Cerebro-cortical Afferent and Efferent Tracts." By DAVID FERRIER, M.D., F.R.S., Professor of Neuropathology, and WILLIAM ALDREN TURNER, M.D., F.R.C.P., Demonstrator of Neuropathology, King's College, London.

II. "On the Relative Behaviour of the H and K Lines of the Spectrum of Calcium." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS.

III. "Further Observations of Enhanced Lines." By J. NORMAN LOCKYER, C.B., F.R.S.

IV. "The Total Solar Eclipse of August 9, 1896. Report on the Expedition to Kiö Island." By J. NORMAN LOCKYER, C.B., F.R.S.

V. "On the Classification of Stars of the δ Cephei Class." By J. NORMAN LOCKYER, C.B., F.R.S.

VI. "On the Appearance of the Cleveite and other New Gas Lines in the Hottest Stars." By J. NORMAN LOCKYER, C.B., F.R.S.

VII. "On the Action exerted by certain Metals and other Substances on a Photographic Plate." By W. J. Russell, Ph.D., F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital.

VIII. "Stress and other Effects produced in Resin and in a Viscid Compound of Resin and Oil by Electrification." By J. W. Swan, F.R.S.

IX. "On Lunar and Solar Periodicities of Earthquakes." By Arthur Schuster, F.R.S.

X. "Cathode Rays and some analogous Rays." By Silvanus P. Thompson, D.Sc., F.R.S.

XI. "Micro-physical Structure of pure Gold and Micro-segregation in Gold Alloys." By Thomas Andrews, F.R.S.

XII. "A Maya Calendar Inscription, interpreted by Goodman's Tables." By Alfred P. Maudslay. Communicated by F. Ducane Godman, F.R.S.

XIII. "Studies in the Morphology of Spore-producing Members. Part III. Marattiaceæ." By F. O. Bower, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow.

XIV. "The Vector Properties of Alternating Currents and other Periodic Quantities." By W. E. Sumpner, D.Sc. Communicated by O. Henrici, F.R.S.

XV. "On the Theory of the Magneto-Optic Phenomena of Iron, Nickel, and Cobalt." By J. G. Leathem, B.A., Fellow of St. John's College, Cambridge. Communicated by Sir Robert S. Ball, F.R.S.

XVI. "Some Observations on the Chemistry of the Contents of the Alimentary Tract under various conditions; and on the Influence of the Bacteria present in them." By A. Lockhart Gillespie, M.D., F.R.C.P.(Ed.), F.R.S.E. Communicated by Professor J. G. McKendrick, F.R.S.

XVII. "On a Discontinuous Variation occurring in *Biscutella lævigata*." By Miss E. R. Saunders, Lecturer at Newnham College, Cambridge. Communicated by W. Bateson, F.R.S.

XVIII. "Magnetic Properties of Iron at High Temperatures." By J. Hopkinson, D.Sc., F.R.S.

XIX. "On the Change of Absorption produced by Fluorescence." By John Burke, B.A.(Dub.), Berkeley Fellow of the Owens College, Manchester. Communicated by Professor Arthur Schuster, F.R.S.

XX. "Influence of Acids and Alkalis upon the Electrotonic Currents of Medullated Nerve." By AUGUSTUS W. WALLER, M.D., F.R.S.

XXI. "On the Distribution of Frequency (Variation and Correlation) of the Barometric Height at diverse Stations." By KARL PEARSON, M.A., F.R.S., University College, London, and Miss ALICE LEE, Bedford College.

XXII. "On the Openings in the Wall of the Body-cavity of Vertebrates." By E. J. BLES. Communicated by Dr. HANS GADOW, F.R.S.

XXIII. "Electrification of Air, of Vapour of Water, and of other Gases." By LORD KELVIN, G.C.V.O., F.R.S., MAGNUS MACLEAN, D.Sc., F.R.S.E., and ALEXANDER GALT, B.Sc., F.R.S.E.

XXIV. "Note on recent Investigations on the Mechanical Equivalent of Heat." By E. H. GRIFFITHS, M.A., F.R.S.

The Society adjourned over the Long Vacation to Thursday, November 18.

"An Investigation on the Variability of the Human Skeleton with especial Reference to the Naqada Race, discovered by Professor Flinders Petrie in his Explorations in Egypt." By ERNEST WARREN, B.Sc. Communicated by Professor W. F. R. WELDON, F.R.S. Received April 2,—Read June 3, 1897.

(Abstract.)

Charles Darwin was the first to point out the great importance of the study of the nature and causes of variation. Since his time, two methods of investigation have been adopted; in the one the obvious and strongly pronounced variations are selected and the probability of species having been derived from such "sports" is analysed and discussed. According to the second method a population of a species is taken, and the condition of the organs in each individual is determined. It is believed that this latter statistical method will lead to more accurate ideas as to the causes of variation and as to the part which variation has played in the origin of species.

As in all other scientific investigations, the course pursued by statistical inquiry is, firstly, to record facts, and, secondly, to generalise from the facts. The facts consist of the nature of the

variation and the relationships which obtain between one variation and another; these may be expressed by diagrams or by means of a few numerical constants. After the data have thus been expressed in a convenient form, they are examined to see if any legitimate theory or generalisation can be drawn from them. If not, the facts nevertheless possess a value, for light is sure to be shed on them by future investigations, since the subject of variation is still in its infancy.

To obtain an accurate knowledge of the variability displayed by man and the mutual relationships of the different organs is surely a subject of great interest and of both practical and theoretical importance. An exceptionally favourable opportunity for studying the variation exhibited by the human skeleton in a fairly homogeneous race was presented by the kindness of Professor Petrie in placing at my disposal the skeletons of the "New Race" which he has recently brought over from Egypt. There were remains of about 400 skeletons.

Although numerous bone-measurements have been made, yet generally the number in each race has been too small to be of any statistical use, and even when fairly numerous measurements have been made on a single race, the results have been dealt with in a very inadequate manner. So far as I am aware, the present investigation is the first where the limb-bones have been measured for statistical purposes, and it is also, I believe, the largest series of measurements that have been made on the human skeleton.

The paper deals primarily with the nature of the variation in the length of the limb-bones and of the correlation between the bones. The sacrum and pectoral girdle have also been studied as far as the material would allow. Besides this I have made some observations on the more important ethnological characters which the material exhibited.

The measurements of each bone are recorded in the tables, and these constitute the foundation for all theory, and in the future will be of use for discovering fresh relationships and for correcting any erroneous ideas.

In the following paragraphs some of the more important conclusions are briefly recapitulated:—

(1) The variability of the limb-bones is roughly proportional to their absolute length, and hence expressing the variability in terms of the mean we tend to get a constant ratio.

The ratio $\frac{\text{standard deviation} \times 100}{\text{mean}}$ fluctuates about 5. We do not admit that this ratio (called the "coefficient of variation") always expresses the variability of an organ as it concerns the individual.

(2) The mode of dispersion of the variations about the mean would appear to be distinctly skew, and the range given by the theoretical curves would seem to be generally limited. The frequency curves are steeper on the negative side than on the positive side of the origin, hence there is greater variability towards long bones than towards short bones, that is, abnormality in the direction of giants is greater than in the direction of dwarfs.

(3) The femora of the New Race were strongly pilastric, occasionally they were platymeric, but then the pilastric index* was generally low. This observation is in accordance with Professor Manouvrier's statement that platymerism is more frequently exhibited in femora with low than with high pilastric indices.

(4) The length of the head + neck of the femur in proportion to the total length of the bone is greater in man than in woman. I find that in the New Race the angle of the neck with the shaft is larger in woman than in man, though on somewhat scant data the reverse is stated to be the case in Europeans.

(5) In the New Race the angle of torsion of the femur is much greater than in Europeans; it is suggested that this angle may be found to be a race character.

(6) The angle which the condyles of the femur make with the horizontal plane, when the bone is held upright in the maximum position, is greater in woman than in man. This would appear to be due to the greater width of the pelvis. It is suggested that this angle might yield valuable information as to the width of the pelvis in prehistoric peoples.

(7) The tibiæ exhibited platycnemia to a marked degree. A correlation is believed to exist between the flattening of the tibia and the condition of the pilastre of the femur; hence as the pilastric condition would seem to be produced by a vigorous musculature platycnemia may probably be referred to a similar cause, and so it is not an atavistic character.

(8) Perforation of the septum between the olecranon and coronoid fossæ of the humerus was very frequent. Also it occurred more often in the female than in the male, and on the left side than on the right. It is suggested that in man this perforation is frequently incidental, and is due to the impact of the beak of the olecranon against the septum in extension of the arm.

(9) Many of the ulnæ were conspicuously incurved. The olecranon was remarkable for its variation in size.

(10) The femur + tibia is longer on the left side than on the right, while the right humerus + radius is very considerably greater

* Pilastric index $= \frac{\text{antero-posterior diameter} \times 100}{\text{diameter from right to left}}$. The measurements are taken at the middle of the shaft.

than the left. The difference between the means of the two sides would appear to vary in different races; also it is distinctly greater in woman than in man.

(11) The absolute correlation between the lengths of the long bones is high, while, on the other hand, the correlation between length and breadth would appear to be weak.

(12) On comparing with the Aino it would seem that serially homologous bones are more strongly correlated with one another than are non-homologous bones. Also the femur and tibia are more closely correlated than the humerus and radius. Distal bones would appear to be somewhat less correlated with one another than are proximal bones, *e.g.*, there is slightly less correlation between the tibia and radius than between the femur and humerus.

In the New Race woman was distinctly less correlated than man.

(13) Sometimes it is desirable to refer the variations to some standard, such as stature. The index-correlations tend to be lower than the absolute correlations. From the results we have obtained it appears doubtful as to what meaning we can attach to the difference between the observed correlation of ratios and Professor Pearson's "spurious correlation."

(14) The New Race approached the Negro in the proportional lengths of the limb-bones to one another, but the sacral and scapular indices were nearly identical with those of Europeans.

"The average Contribution of each several Ancestor to the total Heritage of the Offspring." By FRANCIS GALTON, D.C.L., Sc.D., F.R.S. Received and Read June 3, 1897.

In the following memoir the truth will be verified in a particular instance, of a statistical law of heredity that appears to be universally applicable to bisexual descent. I stated it briefly and with hesitation in my book 'Natural Inheritance' (Macmillan, 1889; page 134), because it was then unsupported by sufficient evidence. Its existence was originally suggested by general considerations, and it might, as will be shown, have been inferred from them with considerable assurance. Consequently, as it is now found to hold good in a special case, there are strong grounds for believing it to be a general law of heredity.

I have had great difficulty in obtaining a sufficient amount of suitable evidence for the purpose of verification. A somewhat extensive series of experiments with moths were carried on, in order to supply it, but they unfortunately failed, partly owing to the diminishing fertility of successive broods and partly to the large disturbing effects of differences in food and environment on different

broods and in different places and years. No statistical results of any consistence or value could be obtained from them. Latterly, while engaged in planning another extensive experiment with small, fast-breeding mammals, I became acquainted with the existence of a long series of records, preserved by Sir Everett Millais, of the colours during many successive generations of a large pedigree stock of Basset hounds, that he originated some twenty years ago, having purchased ninety-three of them on the Continent, for the purpose. These records afford the foundation upon which this memoir rests.

The law to be verified may seem at first sight too artificial to be true, but a closer examination shows that prejudice arising from the cursory impression is unfounded. This subject will be alluded to again, in the meantime the law shall be stated. It is that the two parents contribute between them on the average one-half, or $(0{\cdot}5)$ of the total heritage of the offspring; the four grandparents, one-quarter, or $(0{\cdot}5)^2$; the eight great-grandparents, one-eighth, or $(0{\cdot}5)^3$, and so on. Thus the sum of the ancestral contributions is expressed by the series $\{(0{\cdot}5)+(0{\cdot}5)^2+(0{\cdot}5)^3$, &c.$\}$, which, being equal to 1, accounts for the whole heritage.

The same statement may be put into a different form, in which a parent, grandparent, &c., is spoken of without reference to sex, by saying that each parent contributes on an average one-quarter, or $(0{\cdot}5)^2$, each grandparent one-sixteenth, or $(0{\cdot}5)^4$, and so on, and that generally the occupier of each ancestral place in the nth degree, whatever be the value of n, contributes $(0{\cdot}5)^{2n}$ of the heritage.

In interbred stock there are always fewer, and usually far fewer, different individuals among the ancestry than ancestral places for them to fill. A pedigree stock descended from a single couple, m generations back, will have 2^m ancestral places of the mth order, but only two individuals to fill them; therefore if $m = 10$ there are 1024 such places; if $m = 20$ there are more than a million. Whenever the same individual occupies many places he will be separately rated for each of them.

The neglect of individual prepotencies is justified in a law that avowedly relates to average results; they must of course be taken into account when applying the general law to individual cases. No difficulty arises in dealing with characters that are limited by sex, when their equivalents in the opposite sex are known, for instance in the statures of men and women.

The law may be applied *either* to total values or to deviations, as will be gathered from the following equation. Let M be the mean value from which all deviations are reckoned, and let D_1, D_2, &c., be the means of all the deviations, including their signs, of the ancestors in the 1st, 2nd, &c., degrees respectively; then

$$\tfrac{1}{2}(M+D_1)+\tfrac{1}{4}(M+D_2)+\&c. = M+(\tfrac{1}{2}D_1+\tfrac{1}{4}D_2+\&c.)$$

It should noted that nothing in this statistical law contradicts the generally accepted view that the chief, if not the sole, line of descent runs from germ to germ and not from person to person. The person may be accepted on the whole as a fair representative of the germ, and, being so, the statistical laws which apply to the persons would apply to the germs also, though with less precision in individual cases. Now this law is strictly consonant with the observed binary subdivisions of the germ cells, and the concomitant extrusion and loss of one-half of the several contributions from each of the two parents to the germ-cell of the offspring. The apparent artificiality of the law ceases on these grounds to afford cause for doubt; its close agreement with physiological phenomena ought to give a prejudice in *favour* of its truth rather than the contrary.

Again, a wide though limited range of observation assures us that the occupier of each ancestral place *may* contribute something of his own personal peculiarity, apart from all others, to the heritage of the offspring. Therefore there is such a thing as an average contribution appropriate to each ancestral place, which admits of statistical valuation, however minute it may be. It is also well known that the more remote stages of ancestry contribute considerably less than the nearer ones. Further, it is reasonable to believe that the contributions of parents to children are in the same proportion as those of the grandparents to the parents, of the great-grandparents to the grandparents, and so on; in short, that their total amount is to be expressed by the sum of the terms in an infinite geometric series diminishing to zero. Lastly, it is an essential condition that their total amount should be equal to 1, in order to account for the whole of the heritage. All these conditions are fulfilled by the series of $\frac{1}{2}+\frac{1^2}{2}+\frac{1^3}{2}+$ &c., and by no other. These and the foregoing considerations were referred to when saying that the law might be inferred with considerable assurance *à priori*; consequently, being found true in the particular case about to be stated, there is good reason to accept the law in a general sense.

The Bassets are dwarf blood-hounds, of two, and only two, recognised varieties of colour. Excluding, as I have done, a solitary exception of black and tan, they are either white, with large blotches ranging between red and yellow, or they may in addition be marked with more or less black. In the former case they are technically known and registered as "lemon and white," in the latter case as "tricolour." Tricolour is, in fact, the introduction of melanism, so I shall treat the colours simply as being "tricolour" or "non-tricolour;" more briefly, as T. or N. I am assured that transitional cases between T. and N. are very rare, and that experts would hardly ever disagree about the class to which any particular hound should be assigned. A stud-book is published from time to time

containing the pedigrees, dates of birth, and the names of the breeders of these valuable animals. The one I have used bears the title 'The Basset Hound Club Rules and Stud-Book,' compiled by Everett Millais, 1874–1896. It contains the names of nearly 1000 hounds, to which Sir Everett Millais has very obligingly, at my request, appended their colours so far as they have been registered, which during later years has almost invariably been done. The upshot is that I have had the good fortune to discuss a total of 817 hounds of known colour, all descended from parents of known colour. In 567 out of these 817, the colours of all four grandparents were also known. These two sets are summarised in Table I and discussed in Table V, and they afford the data for Tables II, III, and IV. In 188 of the above cases the colours of all the eight great-grandparents were known as well; this third set is discussed in Table VI.

Partly owing to inequality in the numbers of the tricolours and non-tricolours, and partly owing to a selective mating in favour of the former, the different possible combinations of T. and N. ancestry are by no means equally common. The effect of this is conspicuous in Table I, where the entries are huddled together in some parts and absent in others. Still, though the data are not distributed as evenly as could be wished, they will serve our purpose if we are justified in grouping them without regard to sex; or, more generally, if we treat the 2^n components of each several A_n, whatever be the value of n, as equally efficient contributors.

Our first inquiry then must be, "Is or is not one sex so markedly prepotent over the other, in transmitting colour, that a disregard of sex would introduce statistical error?" In answering this, we should bear in mind a common experience, that statistical questions relating to sex are very difficult to deal with. Large and unknown disturbing causes appear commonly to exist, that make data which are seemingly homogeneous, very heterogeneous in reality. Some of these are undoubtedly present here, especially such as may be due to individual prepotencies combined with close interbreeding. For although this pedigree stock originated in as many as ninety-three different hounds, presumably more or less distant relations to one another, some of them proved of so much greater value than the rest that very close interbreeding has subsequently been resorted to in numerous instances. In order to show the danger of trusting blindly to averages of sex, even when the numbers are large, I have compared the results derived from different sets of data, namely from those contained in the last two columns of Table I, where they are distinguished by the letters A and B, and have treated them both separately and together in Table II. They will be seen to disagree widely, concurring only in showing that the dam is prepotent over the sire in transmitting colour. According to the A data, their

relative efficacy in this respect is as 58 to 51, say 114 to 100; according to the B data, it is as 47 to 32, say 147 to 100. Taking all the data together, it is as 54 to 45, say 120 to 100, or as 6 to 5.

It does not seem to me that this ratio of efficacy of 6 to 5 is sufficient to overbear the statistical advantages of grouping the sexes as if they were equally efficient, the error in one case being more or less balanced by an opposite error in the other. It is true that the reciprocal forms of mating are by no means equally numerous, the prevailing tendency to use tricolours as sires being conspicuous. Still, as will be found later, on the application of a general test, the error feared is too insignificant to be observed. Should, however, a much larger collection of these data be obtained hereafter, minutiæ ought to be taken into account which may now be disregarded, and the neglect of female prepotency would cease to be justified.

The law to be verified supposes all the ancestors to be known, or to be known for so many generations back that the effects of the unknown residue are too small for consideration. The amount of the residual effect, beyond any given generation, is easily determined by the fact that in the series $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}$, &c., each term is equal to the sum of all its successors. Now in the two sets of cases to be dealt with the larger refers to only two generations, therefore as the effect of the second generation is $\frac{1}{4}$, that of the unknown residue is $\frac{1}{4}$ also. The smaller set refers to three generations, leaving an unknown residual effect of $\frac{1}{8}$. These large residues cannot be ignored, amounting, as they do, to 25 and 12·5 per cent. respectively. We have, therefore, to determine fixed and reasonable rules by which they should be apportioned.

The requisite data for doing this are given in Table III, which shows that 79 per cent. of the parents of tricolour hounds are tricolour also, and that 56 per cent. of the parents of non-tricolour hounds are tricolour. It is not to be supposed that the trustworthiness of these results reaches to 1 per cent., but they are the best available data, so I adopt them.

It will be convenient to use the following nomenclature in calculation:—

a_0 stands for a single member of the offspring.

a_1 for a single parent; a_2 for a single grandparent, and so on, the suffix denoting the number of the generation. A parallel nomenclature, using capital letters, is:—

A_0 stands for all the offspring of the same ancestry.

A_1 for the two parents; A_2 for all the four grandparents, and so on. Consequently A_n contains 2^n individuals, each of the form a_n, and A_n contributes $(0{\cdot}5)^n$ to the heritage of each a_0; while each a_n contributes $(0{\cdot}5)^{2n}$ to it.

In the upper part of Table IV the ratios are entered of the average

contributions of T. supplied by *known* ancestors. Nothing further need be said about these, except that they are styled coefficients because they must be multiplied into the total number of offspring, in order to calculate the number of them that will, on their separate and independent accounts, be probably tricolours.

We have next to explain how the coefficients for the *unknown* ancestry have been calculated, namely, those which are entered in the lower part of Table IV. Suppose all the four grandparents, A_2, to be tricolour, then only 0·79 of A_3 will be tricolour also, $(0{\cdot}79)^2$ of A_4, and so on. These several orders of ancestry will respectively contribute an average of tricolour to each a_0 of the amounts of $(0{\cdot}5)^3 \times (0{\cdot}79)$, $(0{\cdot}5)^4 \times (0{\cdot}79)^2$, &c. Consequently the sum of their tricolour contributions is

$$(0{\cdot}5)^3 \times (0{\cdot}79)\{1 + (0{\cdot}5) \times (0{\cdot}79) + (0{\cdot}5)^2 \times (0{\cdot}79)^2 + \&c.\},$$

which equals 0·1632. The average tricolour contribution from the ancestry of *each* of the four tricolour grandparents must be reckoned as the quarter of this, namely, 0·0408.

By a similar process, the average tricolour contribution from the ancestry of *each* non-tricolour grandparent is found to be 0·0243.

When the furthest known generation is that of the great-grandparents, the formula differs from the foregoing only by substituting $(0{\cdot}5)^4 \times (0{\cdot}79)$ for $(0{\cdot}5)^3 \times (0{\cdot}79)$. This makes the average tricolour contribution from the ancestry of the whole eight tricolour great-grandparents equal to 0·08160, and that from the ancestry of *each* of them to be one-eighth of this, or 0·0102.

In a similar way the tricolour contribution from the ancestry of *each* non-tricolour great-grandparent is found to be 0·0061.

The following example shows how the coefficients in Table IV were utilised in calculating the general coefficients entered in Table V.

2 Parents, T_1 (personal)	0·5000
3 Grandparents, T_2 (personal)	0·1875
1 Grandparent, N_2 (personal)	—
3 Grandparents, T_2 (ancestral).....	0·1224
1 Grandparent, N_2 (ancestral)	0·0243
Total tricolour contribution ..	0·8342

The coefficient 0·83 will consequently be found under the appropriate head in Table V, where the total number of offspring ("all cases") is recorded as 119. By multiplying these together, viz., $0{\cdot}83 \times 119$, the "calculated" number of 99 is obtained. It will be seen that the observed number was 101, a difference of only 2 per cent.

The extraordinarily close coincidence throughout the two tables,

V and VI, between calculation and observation, proves that the law is correct in the present instance, and that the principle by which the unknown ancestry was apportioned, is practically exact also. It is not so strictly exact as it might have been, because the whole of the available knowledge has not been utilised. The 0·79 applied to A_4, &c., requires some small correction according to the known colours of the *offspring* of A_3. If they had been all tricolour the 0·79 would have to be increased; if all non-tricolour, it would have to be diminished. Having insufficient data to check a theoretical emendation, I note its omission, but shall not discuss the matter further.

It will be easily understood from these remarks how *collateral* data are to be brought into calculation, for if the collaterals were more tricolour than the average of hounds, the 0·79 would have to be somewhat increased (but not beyond the limiting value of 1·00); if less tricolour than the average, the 0·79 would have to be diminished. The knowledge of collaterals would be superfluous, if that of the direct ancestry were complete, but this important prolongation of the present subject must not be considered further on this occasion.

There are three stages in Tables V and VI at which comparisons may be made between calculated results and observed facts.

(1) *The Grand Totals.*—In Table V the sum of all the calculated values amounts to 391; that of all the observed ones to 387, which are closely alike. In Table VI they amount to 180 and 181 respectively, which is a still closer resemblance. Consequently the calculations are practically exact *on the whole*, and the error occasioned by neglect of sex, &c., is insignificant.

(2) *The Subordinate Pairs of Totals.*—These are entered at the sides of the tables, and are nine in number, namely, 236, 239; 149, 139; 6, 9; 53, 56; 52, 56: 9, 9; 8, 6; 49, 46; 9, 8. The coincidences are striking, in comparison with such results as statisticians have usually to be contented with; the second pair, 149, 139, is the least good, and will be considered in the next paragraph.

(3) *Individual Pairs of Entries.*—There are 32 of these; here also calculation compares excellently well with observation, excepting in the line that furnishes the "subordinate totals" of 149, 139, where the "all cases" of 37, 158, 60 yield the tricolour contingents of 20, 79, 36. Dividing each tricolour by the corresponding "all cases," we obtain what may be called "Coefficients from Observation," to compare with the calculated coefficients. They are as follows:—

		Diff.		Diff.	
Coefficients from observation	54	(−4)	50	(+10)	60
„ „ calculation	66	(−8)	58	(−7)	51

The great irregularity of the entries in the upper line shows that the observed values cannot be accepted as true representa-

tives of the normal condition. I have not unravelled the causes of this error, and it is not urgent to do so, since its ill effects are swamped by the large number of successes elsewhere.

In order to satisfy myself that the correspondence between calculated and observed values was a sharp test of the correctness of the coefficients, I made many experiments by altering them slightly, and recalculating. In every case there was a notable diminution in the accuracy of the results. The test that the theory has successfully undergone appeared on that account, to be even more searching and severe than I had anticipated.

It is hardly necessary to insist on the importance of possessing a correct law of heredity. Vast sums of money are spent in rearing pedigree stock of the most varied kinds, as horses, cattle, sheep, pigs, dogs, and other animals, besides flowers and fruits. The current views of breeders and horticulturists on heredity are contradictory in important respects, and therefore *must* be more or less erroneous. Certainly no popular view at all resembles that which is justified by the present memoir. A correct law of heredity would also be of service in discussing actuarial problems relating to hereditary longevity and disease, and it might throw light on many questions connected with the theory of evolution.

Table I.—Pedigrees of Parental and Grand Parental Colours (Tricolour (T) or Non-Tricolour (N)).

The Sex of the Ancestors is taken into Account, but not that of the Offspring.

Sire's sire / Sire's dam			T T	T T	T N	T N	T T	T T	T N	T N	N T	N T	N N	N N	N T	N T	N N	N N	Totals.	Others of which all G. P.'s are not known.
Dam's sire / Dam's dam			T T	T N	T T	T N	N T	N N	N T	N N	T T	T N	T T	T N	N T	N N	N T	N N		
Sire.	Dam.	Offspring.	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*j*	*k*	*l*	*m*	*n*	*o*	*p*	A.	B.
Tric.	Tric. ..	Tric.	106	38	47	12	12	7	2	4	3	..	3	4	..	..	..	..	239	87
		Non-T.	13	4	12	2	2	1	1	..	..	..	..	3	..	..	..	..	38	20
Tric.	Non-T.	Tric.	..	..	16	3	..	..	1	..	3	..	..	..	1	1	..	..	25	15
		Non-T.	..	..	6	1	..	..	3	..	2	..	..	4	1	1	..	..	18	17
Non-T. ..	Tric. ..	Tric.	20	45	4	20	11	4	4	..	..	2	..	3	1	..	..	..	114	30
		Non-T.	17	50	16	9	5	..	1	1	..	8	.	1	1	..	..	..	109	64
Non-T. ..	Non-T.	Tric.	..	..	..	2	..	..	..	..	..	5	..	1	..	1	..	..	9	11
		Non-T.	..	..	..	8	..	..	..	4	..	3	..	..	..	..	..	..	15	6
Totals			156	138	101	57	30	12	12	9	8	18	3	16	4	3	..	..	567	250

Table II.—Offspring of one parent Tricolour (T) and of one Non-tricolour (N). The sex of the parents is not regarded.

From Table I.		Observed.			Per cents.	
		Tricolour.	Non-tricolour.		Tricolour.	Non-tricolour.
Sire T, dam N	A	114	109	223	51	49
	B	30	64	94	32	68
	Sum	144	173	317	45	55
Dam T, sire N	A	25	18	43	58	42
	B	15	17	32	47	53
	Sum	40	35	75	54	46

Table III.—Distribution of T and N colour in Parents, when the Offspring are T and N respectively.

From Table I.		No. of T offspring.			Parents* of T offspring.	
Sires.	Dams.	A.	B.	Total.	T.	N.
T	T	239	87	326	652	0
T	N	25	15	40	40	40
N	T	114	30	144	144	144
N	N	9	11	20	0	40
Totals				530	836	224
Per cent. of parents*				50	79	21

From Table I.		No. of N offspring.			Parents* of N offspring.	
Sires.	Dams.	A.	B.	Total.	T.	N.
T	T	38	20	58	116	0
T	N	18	17	35	35	35
N	T	109	64	173	173	173
N	N	15	6	21	0	42
Totals				287	324	250
Per cent. of Parents*				50	56	44

* More properly "Parental Places"; the number of these, though not that of the individual parents, being always double the number of any group of offspring.

ERRATUM.

Vol. 61, p. 410.

In the memoir read June 3, 1897, entitled "The average Contribution of each several Ancestor to the total Heritage of the Offspring" the words *sire* and *dam* have been accidentally transposed in Table II, and consequently, in the deduction therefrom at the bottom of page 404, the latter should be that, in the present case, the sire is more potent in transmitting colour than the dam, in the ratio of 6 to 5. This error does not affect the general conclusions of the memoir, because the ratio of 6 to 5 was treated as an insignificant disproportion, and the two sexes were dealt with on equal terms.

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Table IV.—Tricolour coefficients.

	Ancestry known up to and inclusive of	
	Grandparents.	Great-grandparents.
Personal allowance of T for each		
Tricolour parent	0·2500	0·2500
„ grandparent	0·0625	0·0625
„ great-grandparent	—	0·0156
(No allowance for Non-tricolours.)	—	—
Ancestral allowance of T for each		
Tricolour grandparent	0·0408	—
Non-tricolour „	0·0243	—
Tricolour great-grandparent	—	0·0102
Non-tricolour „ „	—	0·0061

Table V.—Calculation and Observation Compared.

The pedigrees are utilised up to the second ascending generation. Sex not taken into account.

No. of tricolours in parents.		Number of tricolours in grandparents.				Total tricolour offspring.	
		4	3	2	1	Calculated.	Observed.
		a	*bcei*	*dfgjkm*	*hlno*		
2	All cases	119	119	28	11		
	Coefficient	0·91	0·83	0·76	0·68		
	Tricolour calc'd.	*108*	*99*	*21*	*8*	*236*	
	„ observed	106	101	24	8		239
1	All cases	37	158	60	6		
	Coefficient	0·66	0·58	0·51	0·43		
	Tricolour calc'd.	*24*	*92*	*30*	*3*	*149*	
	„ observed	20	79	36	4		139
0	All cases	..	..	18	6		
	Coefficient	..	..	0·26	0·18		
	Tricolour calc'd.	..	..	*5*	*1*	*6*	
	„ observed	..	..	7	2		9
					Grand totals	*391*	387

Table VI.—Calculation and Observation Compared.

The pedigrees are utilised up to the third ascending generation.
Sex not taken into account.

No. of tricolours in parents.	No. of tricolours in grandparents.		Number of tricolours in great-grandparents. 8	7	6	5	4	Total tricolour offspring. Calculated.	Observed
2	4	All cases	2	25	14	16			
		Coefficient.....	0·96	0·94	0·92	0·90			
		Tricolours calc'd.	*2*	*24*	*13*	*14*	..	*53*	
		„ observed	2	25	14	15	..		56
	3	All cases	..	18	21	16	6		
		Coefficient.....	..	0·87	0·85	0·83	0·81		
		Tricolours calc'd.	..	*16*	*18*	*13*	*5*	*52*	
		„ observed	..	17	19	14	6		56
	2	All cases	..	3	2	3	3		
		Coefficient.....	..	0·81	0·79	0·77	0·75		
		Tricolours calc'd.	..	*2*	*3*	*2*	*2*	*9*	
		„ observed	..	2	2	3	2		9
1	4	All cases	..	2	1	9			
		Coefficient.....	..	0·69	0·67	0·65			
		Tricolours calc'd.	..	*1*	*1*	*6*	..	*8*	
		„ observed	..	1	..	5	..		6
	3	All cases	1	28	14	31	9		
		Coefficient.....	0·64	0·62	0·60	0·58	0·56		
		Tricolours calc'd.	*1*	*17*	*8*	*18*	*5*	*49*	
		„ observed.	1	16	12	8	9		46
	2	All cases	..	..	4	13			
		Coefficient.....	..	..	0·54	0·52			
		Tricolours calc'd.	..	..	*2*	*7*	..	*9*	
		„ observed	..	..	1	7	..		8
							Grand totals ..	*180*	181

The summed data derived from Table IV, form the coefficients entered in Tables V and VI. These are multiplied into the corresponding number of "all cases," and the result gives the "calculated" number of tricolour hounds among them.

The entries of "all cases" and of "tricolours observed" in Table V are deduced from Table I, by combining the appropriate columns. The letters at the top show which columns are combined.

Seven other observed cases, disposed in three groups, are scattered beyond the limits of Table VI; two of these seven cases are tricolour.

"Further Note on the Influence of a Magnetic Field on Radiation Frequency." By Oliver Lodge, F.R.S., assisted by Mr. Benjamin Davies. Received May 19,—Read June 3, 1897.

Referring to a former communication of mine, on the subject of Zeeman's discovery, printed on page 513 of the 'Proceedings of the Royal Society' for February 11 this year, vol. 60, No. 367, I wish to add an observation to those previously recorded, as I have recently acquired a concave Rowland grating ($3\frac{1}{2} \times 1\frac{1}{2}$-inch ruled surface, 14,438 lines to inch, 10 feet radius of curvature, being the one used by Mr. George Higgs), of which the spectra of the first and third orders on one side are very satisfactory.

It is said on page 513, "If the focussing is sharp enough to show a narrow, dark reversal line down the middle of each sodium line, that dark line completely disappears when the magnet is excited." With the greater optical power now available the dark reversal line is often by no means narrow, and though in some positions of the flame it does still tend to disappear or become less manifest when the flame is subjected to a concentrated magnetic field, the reason of its partial disappearance is that it is partially reversed again—*i.e.*, that a third bright line, as it were, makes its appearance in the midst of the dark line, giving a triple appearance to each sodium line.

More completely stated the phenomena are as follows:—After obtaining each sodium line with a prominently double aspect by manipulating the flame, the magnet is excited, and the dark band in the midst of each sodium line is then seen to widen out considerably in the region of most intense magnetisation, while a bright intrusion line makes its appearance. On closer examination this new line is seen to be double, by reason of a dark division down its middle; and I apprehend that with still more magnetic power this dark band might itself open out into two; but this last phenomenon I have not yet observed.

The whole sodium group is thus seen as if it were octuple. The effect is not due to a mere mechanical disturbance or rearrangement of the gases of the flame by the agency of magnetism; because a

nicol, placed in the rays emanating transversely to the magnetic lines of force, cuts off nearly all the visible magnetic effect when oriented so as to get rid of light whose plane of polarisation contains the lines of force—that is, of oscillations or revolutions whose electrical components are across or around the magnetic lines. That it does not cut off every trace of the effect appears to be due to the fact that the field of force is not strictly uniform, and so its lines are not strictly parallel.

The following is a summary of the different appearances that may be seen according to the state of the flame and the strength of the field:—

At low temperature, and with the flame forward in the field, when each sodium line is sharp and single, magnetism widens it, and with a little more power doubles it, causing a distinct dark line down its middle. The same effect occurs with lithium and thallium lines.

At higher temperature, and with the flame partially behind the field, when each sodium line appears as a broad hazy-edged double, magnetisation greatly widens the doubling, pushing asunder the bright components very markedly; stronger magnetisation reverses the middle of the widened dark band, giving a triple appearance; stronger magnetisation still reverses the middle once more, giving a quadruple appearance to the line. In every case a nicol, suitably placed, cuts off all the magnetic effect and restores the original appearance of the line.

A curious circumstance is that although both lines, D_1 and D_2, show the effect, D_1, *i.e.*, the less refrangible line, shows it best and most sharply. I should describe the effect on D_2 as a coarse widening of considerable amount, but without very clear definition; whereas the widening of D_1, though perhaps no greater in amount, is decidedly better defined. There is no doubt but that, with my grating, D_1 is the line at which one finds oneself usually looking in order to see the details of the change best; and I can hardly suppose this to be subjective to the grating. I hope to show the effects at the soirée next Wednesday.

[The same thing is seen when salts of lithium or of thallium are introduced into the flame; and the components of the doubled red lines are more widely separated than the components of the doubled green lines, the effect being proportional to wave-length. The most interesting line to try was the red cadmium line, since this has been proved to be of specially simple constitution by Michelson. We have recently been able to get the cadmium spectrum well developed by means of a sort of spark arc between the magnet poles, maintained by an induction coil excited by an alternating machine; and we find that the magnetic doubling of the chief lines occurs in

precisely the same way with the spark spectrum as with the flame spectrum, and that the red cadmium line behaves in the same way as the others. The magnetic effect is better seen, from a direction perpendicular to the line of force, when a nicol is interposed in the path of the light, but rotation of the nicol through 90° cuts it entirely off, accurately so when a small spark is the source of light.—May 31.]

Fifth Report to the Royal Society Water Research Committee.

By H. Marshall Ward, Sc.D., F.R.S., F.L.S., Professor of Botany in the University of Cambridge. Presented to the President and Council, December 10, 1896.

(Abstract.)

The following is a short *résumé* of the principal points resulting from three years' study of the Bacterial Flora of the Thames.

All the forms have been grown on gelatine, agar, and potato, as well as in broth, milk, and sugar-solutions, and some of them in special media in addition. Moreover, most of them have been cultivated in hanging drops under the microscope, sometimes isolated and under high powers, for long periods, and the course of development of the colonies traced in detail in several cases, and even when this could not be successfully carried to a conclusion, information as to the changes and growth of the organisms has been obtained, which helps to throw light on their behaviour and relationships.

This work has occupied a long time, and these water bacteria do little beyond growing and dividing, so that in many respects this part of the work only starts problems for the future. In some cases, however, the morphological changes observed are helpful in explaining the macroscopic appearances of the colonies, and in any case it is clear that no examination of a bacterium can be considered complete until its life-history has been traced under the microscope.

The number of forms isolated and cultivated amount to eighty, not counting the large number which were either rejected at an early stage as certainly duplicates, or lost during cultivation.

Of these eighty forms some have been distinctly recognised as well known types, *e.g.*, *Bacillus membranaceus amethystinus* (Eis.), *B. fluorescens liquefaciens* (Fl.), *B. fluorescens non-liquefaciens* (Fl.), *B. coli commune*, *Proteus vulgaris* (Haus.), *Bacillus arborescens* (Fr.), *B. prodigiosus* (Ehr), *B. termo* (Cohn), *B. subtilis*, and *Sarcina lutea*.

Others have been referred with less certainty to less well known forms, such as *Bacterium ureæ* (Jaksch), *B. fulvus* (Zimm.), *B. aureus* (Adam.), *Ascococcus* (Cohn), *Micrococcus carneus* (Zimm.), and *M. candicans* (Fl.).

The remainder are either new, or only doubtfully identified as already described forms, or evidently varieties of some of the foregoing.

During the progress of the cultures, large numbers of coloured drawings were made, with the intention of affording means of identification, but it was found that variations are so numerous and so large, that some study of these variations had to be undertaken.

This led to an investigation of the growth of the colonies in gelatine and other media, and an attempt to explain why, and how far, the colonies vary in culture. This necessitated a careful examination of the factors concerned in the development of the shapes, markings, movements, and so forth of the colonies themselves, and to a classification of the characters furnished by these colonies.

One outcome of the above studies was the conviction that two sets of factors are at work in causing the variations found in the colonies. First, the slight variations in the food-materials, temperature, moisture, &c., which cannot be avoided, however carefully the work is done; and, second, variations in the bacterium cell itself as it comes from the river, owing to the exigencies it has been subjected to during its sojourn there. The water of the river is, in fact, a very dilute and indefinite food-solution, and just as changes occur when we remove a bacterium from broth to milk or to gelatine, so do such result when we transfer from the river to these media, and the changes induced in all cases depend on how long the bacterium has been in the one medium or the other, as well as upon other factors.

The river water is a very poor food medium, and so we cannot be surprised that in many cases the recently isolated bacteria behave as weakened forms; the recognition of these enfeebled varieties suggests explanations of many of the bad "species" in the literature of water bacteria. My work goes to show, not that species cannot be made out, but that the limits of the species are, in most cases, far wider than is assumed in descriptions—in other words, that many so-called species in the books are merely varietal forms, whose characters, as given, are not constant, but depend on treatment. How far this is true for any given case will have to be tested on the particular form in question.

Very slight variations in rapidity of growth of the individual bacterium, its power of liquefaction, pigment production, capacity of fermenting, and so on, lead to comparatively very great differences in the appearance of the colonies formed in a given time on, or in, a medium like gelatine, the composition, aëration, hygroscopicity,

density, &c., of which is changing all the time, and since every variation in temperature, light, air-supply, &c., affects the matter also, I find abundant causes for the variations which otherwise appear so astonishing. It results, therefore, that the attempt to determine species of bacteria by ordinary macroscopic methods leads to difficulties of the same kind as would be met if we tried to differentiate species from the marks presented by masses of trees in forests from a distance—say in a balloon. A forest of a given species of tree would appear very different at different seasons, and according to its age, the kind of soil, climate, and so on, and the treatment it had received previous to planting.

In order to emphasise my own impressions of the specific value of the forms isolated, and at the same time leave it clear what are the differences between the various forms or varieties themselves, I have grouped them into types. Each group contains a type which I regard as probably a species, of which the other forms included are varieties. These varieties are fully described and figured, and would certainly be regarded as species in most books consulted. The reasons for grouping them as I have done are fully discussed with the literature under each. Here, of course, I can only give a very brief summary of the groups as follows :—

Group I is made to contain a form which appears to be identical with the *Bacterium ureæ* of Jaksch, and forming curiously characteristic dull stearine-like plate colonies. It was not uncommon in the river, but the want of salient features rendered it comparatively uninteresting, and no prolonged study of other varieties of it was undertaken.

Group II was constituted for a type which in certain cultures developes an intense violet colour exactly like that of a strong solution of gentian violet. It was not common—at any rate in the typical form—but it was easily cultivated, and two varieties were isolated and kept under observation for many months.

The type accords best with Jolles and Eisenberg's *B. membranaceus amethystinus*, and forms the deep blue violet zooglœa-membrane described for that species, but three years' study of the variations in culture convinces me that several violet bacilli described in the literature are identical with this, or at least cannot be separated by the characters given.

A marked tendency to lose its power of forming pigment, and to grow as a pure white form, characterises this bacillus. It is the species with which I obtained the bacterial photographs exhibited in 1894, and is very sensitive to light. The spectroscopic characters of the pigment were determined, and the bacillus itself cultivated under high powers.

Group III comprises the common *B. fluorescens liquefaciens*, very

abundant in the river. Four varieties were isolated and cultivated. It is the same as that previously described by P. Frankland as *B. viscosus*, and by Schmolck as *B. fluorescens nivalis*. Some of the varieties liquefied very feebly or not at all, suggesting that *B. fluorescens non-liquefaciens* may be only a form of the present species, a conclusion borne out by comparing the two side by side, on, and in, various media.

Group IV comprises the non-liquefying forms of the type *B. fluorescens non-liquefaciens*, extremely common in the river, and of which eight were isolated and kept in culture.

These eight varieties differ in details, but the differences are not constant, and their behaviour suggests the identity of several so-called species of green fluorescing bacteria, hitherto regarded as distinct, *e.g.*, the Bacillus of Lesage, that of Adametz, Lustig's *B. aquatilis fluorescens*, Flugge's *B. fluorescens putidus*, and certain forms described by Frick and Zimmermann.

The fact that some of these varieties were pathogenic and some not, points to the possibility of there being two species, but as experience shows that pathogenicity varies as well as other characters, this could not be relied on, any more than the differences in size, colour, &c.

Group V embraces a large series of forms comprising the typical *B. coli communis* and a number of varietal and allied forms. In all sixteen varieties were studied in detail, some of them very closely and for long periods. They are common in the river, especially in summer, though some of the forms show variations so marked that few observers would be likely to suspect their close relationships. For instance, varieties occurred incapable of coagulating milk; others which formed no gas-bubbles in gelatine; others not pathogenic for guinea-pigs. Some other forms, suspiciously like *B. coli* in other respects, but forming capsules, or liquefying gelatine to a greater or less extent, are placed in different groups in deference to usually received opinion, though I am convinced of their close relationship.

High-power cultures show considerable differences in size of rodlets, and facts were obtained of value in understanding the very great differences observed in the macroscopic characters.

Group VI comprises a series of forms centering around the type of Hauser's *Proteus vulgaris*, and of which eleven forms were isolated and cultivated.

One of the most striking features of this type is its variability as to liquefying power; another is the concurrent variations in motile power of the plate-colonies. I recognised the types named *P. mirabilis* and *P. proteus* by Hauser, and during cultures extending over three years have convinced myself that not only are these and his *P. Zenkeri* merely varieties of the same form differing in liquefying

power, but that they vary in several directions towards powerfully liquefying forms of the *B. termo* type, on the one hand, and yellow chromogenic forms, like Zimmermann's *B. ochraceus* and P. Frankland's *B. arborescens*, on the other.

A careful and prolonged study of the plate-colonies and "wandering islets" was made under high powers, throwing light not only on the movements of these colonies over the surface of the gelatine, but also on the structure of all such colonies as are composed of tresses, zoogloea-clumps, and contoured groups of filaments, motile or not.

Drawings were made illustrating the movements and changes in form of the "wandering islets" at intervals, and showing the individual movements of the bacilli on which these depend. Evidence is also given showing that the differences in size, vigour of movement, and liquefaction, and chromogenic power, depend on the vigour of the organism as it comes from the river, as well as on variations in conditions of culture.

Group VII includes seven forms evidently closely allied to the last group, if really distinct. They are remarkable for their more or less pronounced yellow pigment formation, and are termed the yellow Proteus type.

I have identified some of the varieties here collected together as Zimmermann's *B. radiatus* and *B. ochraceus* and P. Frankland's *B. arborescens*, and there can be little doubt that other yellow chromogenic forms described will have to be included. Among these, a number of "non-liquefying" yellow forms may also have to be counted when they have been sufficiently studied in detail.

Group VIII includes a form which varied so much that I had to make numerous separation cultures at various periods during the two years it was kept in culture, to satisfy myself there was no admixture. Its most characteristic features were the star-like character of the plate-colonies and the presence of a bluish iridescence when held up, but a yellow pigment was also formed in varying degrees of intensity. In other characters it comes near Group IX, on the one hand, and the yellow Proteus type of Group VII on the other, but its peculiarities were so puzzling that I kept it apart.

Group IX comprises a series of golden-yellow liquefying forms, of which five varieties were studied. The type is that of Zimmermann's *B. fulvus* and *B. subflavus*, and, while they differ in liquefying power, all form deep chrome-yellow pigment, like yolk of egg. The group is clearly allied to the yellow Proteus group, and seems to connect P. Frankland's *B. aquatilis* and the true Proteus type with *B. arborescens* and *B. ochraceus*, the differences depending largely on the formation of zoogloea or not.

Group X consists of a series of very common non-liquefying forms, of which three varieties were studied in detail. The type appears

nearest to Adametz's *B. aureus* and P. Frankland's *B. aurantiacus*. They grow very slowly, and seem to be feeble forms. From their behaviour during culture and comparison with the last group, I am compelled to regard these non-liquefying yellow forms as merely weakened varieties of the last group; this is borne out by the tendency to liquefaction shown by some of them after being cultivated for some time, as well as by the enfeeblement of liquefying power shown by some of the older cultures of the yellow Proteus group. It is probable that they are simply species of the Proteus group, so weakened by their prolonged sojourn in the river that they can only be cultivated with difficulty in the new environment of the gelatine media, &c. This being so, the whole of the series included in Groups VI to X inclusive may be varieties of one or two species, since I find that, great as the differences seem between the extreme forms, the varieties studied present characters passing imperceptibly into one another. This result is also in accordance with the fact that so many yellow varieties, impossible to separate satisfactorily, have been isolated from water and described by other observers.

Group XI is made to include a colourless capsuled form, not very common in the river, and reminding one of Cohn's *Ascococcus*. Two sets of cultures of it were carried out in detail, and a very characteristic feature was discovered in the plate-colonies, viz., the presence of curious vermiform zoogloea masses imbedded in the colonies; these are not intruded forms, but merely growth-phases of the same species. So far as can be judged, the "species" seems new.

Group XII comprises a series of yellow capsuled bacilli, not uncommon in the river, and morphologically conforming to the type of Friedländer's bacillus. I cultivated five varieties of it. The most characteristic feature is the formation of extremely tough zoogloea-colonies, often looking like the roe of a fish. There is some variation in the degree of liquefying power, and much difference in consistency and other characters, but the sum of all the diagnoses leaves no doubt that all the forms belong to one species.

Group XIII comprises a series of forms which produce a crimson pigment. The type is that of *B. prodigiosus*, and five varieties were isolated and very closely investigated. Colourless varieties were developed in cultivation, and the pigment again restored by suitable treatment. Weakened forms, and considerable differences in liquefying power were also found. Koch's *B. indicus*, Breunig's *Kieler bacillus*, Frank's *B. ruber*, Dowdeswell's *B. rosaceum metalloides*, and Zimmermann's *B. miniaceus* are all identical with this form, or, at least, can be matched by the varieties arising in culture.

Group XIV comprises a series of rapidly liquefying, colourless forms, very common in the river at all times, and conforming to the type of *B. termo* as amended by Macé. I made a prolonged study of

five varieties, and found considerable differences as regards rapidity of liquefaction, anaërobism, and other characters. The series includes Eisenberg's *B. liquefaciens*, Frankland's *B. liquidus*, and Zimmermann's *B. punctatus* and *B. devorans*, and brings together a large series of incompletely described forms with more or less probability. The type is one of the commonest in the Thames, and a pronounced putrefactive bacterium.

Group XV includes three varieties not uncommon in the river in the summer, and conforming to the type of *B. subtilis* in general behaviour and in the characters of their highly resistant spores, the germination of which was carefully traced. The total behaviour of these varieties points to confusion between the hay bacillus (*B. subtilis*) and the potato bacillus (*B. mesentericus*), and I am driven to the conclusion that these two "species" are either identical or more similar than is usually assumed. In any case both these forms are matched by the varieties in this group, which my examination suggests are varieties of one species. The wrinkled growth on potato cannot be relied upon to separate them.

Group XVI comprises a series of varieties of a yellow *Sarcina* of the type *S. lutea*. I obtained five varieties, showing considerable differences in liquefying power, and it was interesting to find that the *Sarcina* form—"packet form"—is not always maintained. My cultures unite Frankland's *S. liquefaciens*, Lindner's *S. flava*, and Schroeter's *S. lutea* as mere varieties of one and the same species.

Group XVII was made for a rose-pink or cerise-coloured micrococcus of the type of Zimmermann's *M. carneus*, not common, but isolated several times in the winter of 1894—1895. The most interesting point was the discovery that in early stages of division, followed under high powers, it develops as a *Sarcina*, and several facts point to the conclusion that the *Sarcina* form is a mere result of slow development—*e.g.*, in acid media—of *Staphylococcus* forms of *Micrococcus*. My cultures unite *M. carneus* with Maschek's *Coccus ruber*, Flügge's *Micrococcus cinnabareus*, Zimmermann's *M. cinnabarinus*, and certain forms often termed *M. roseus*, as well as Schröter's *Sarcina rosea*, as mere varieties of one form.

Group XVIII is made for a curious form which serial cultures under the high power shows to be no schizomycete at all, but a truly branching micro-organism with acropetal growth and other characters which place it in the category of true fungi. Nevertheless it breaks up into minute coccus-like oidial joints, so similar to a short bacterium or oval coccus that it would inevitably be taken for such if examined by ordinary methods only. I referred to it in 1895 as a "false bacterium," and its history affords an excellent proof of the necessity of microscopic cultures in these investigations. It presents

undoubted resemblances to Löffler's *B. diphtheriæ*. It was not found to be pathogenic to guinea-pigs by Dr. Kanthack.

Group XIX was made to contain a white micrococcus of the type *M. candicans* of Flügge. It is very common in the river, and was kept under observation a long time, with interesting results; for although typically a non-liquefying form, it becomes capable of liquefying gelatine in time, and the plate-colonies and other cultures show variations which lead me to connect *M. candicans* with Zimmermann's *M. concentricus*, Tataroff's "schminkeweisser *Streptococcus*," and some other forms, as mere varieties of one and the same species.

Group XX contains a form only isolated once from the Thames, but studied very thoroughly in 1894. It develops lemon-yellow liquefying colonies on gelatine, and presents close resemblances to *Sarcina lutea*, but it is a fairly large bacillus, and no attempt to convert is into a *Sarcina* form succeeded. All efforts to match this form among those described have failed, though incompletely examined forms by Maschek, Unna and Tommasoli, and Adametz present resemblances, so far as their imperfect diagnoses go. It may be necessary to name it as a new species.

Group XXI contains two varieties of a short oval form, or coccus, principally characterised by the development of a red pigment, varying from pale salmon-pink to deep rosy-scarlet, but also differing in details of shape and adaptation to temperature, and a few other points. Nevertheless it was difficult to give the varieties specific rank on the evidence. I have been unable to identify this form.

Group XXII comprises two forms, not completely investigated, and nearer the type of Group XIV than any others of my groups. Both seem common in the river.

The question of the pathogenicity or otherwise of these forms was naturally raised. and was kindly tested for me in several cases by Dr. Lazarus Barlow, and at much greater length by Dr. Kanthack, through whose hands nearly all the forms have passed. In the following summary pathogenic means pathogenic to guinea-pigs. Pathogenic varieties occurred in Groups IV, V, VI, VIII, XI, XIII, XIV, and XVII, with considerable variation in detail, while the other groups seem to be harmless. The most markedly pathogenic forms were in Group V (type of *B. coli commune*). Those in Group IV aroused the suspicion that *B. pyocyaneus* comes here; and it is interesting to note that *B. prodigiosus* (Group XIII) was pathogenic in some cases.

Of course no full test of pathogenicity is afforded by the few trials made in this way, but the positive results obtained at least show that a not inconsiderable number of the river bacteria are pathogenic, or can become so in culture. Not less important is the

fact that pathogenicity, like the other characters, is variable, as is abundantly shown by Dr. Kanthack's experiments.

In conclusion, the results show that—

1. Very many forms occur in the Thames, some of which are pathogenic under certain conditions.

2. The "species" of the descriptive hand-books—principally medical—are frequently not species at all, in the botanical sense, but varieties, or growth-forms, the distinctive characters of which are not constant. These so-called species need revision and grouping around types, which may turn out to be the true species.

3. The characters derived from the behaviour of colonies are not sufficient for the determination of species, and how far they may be employed in conjunction with other characters will only be elucidated by advances in our knowledge of the way the colonies are built up by the growing bacteria on the given media.

4. The effects of definite changes in the environment on the media are of great importance, but have hardly been noticed as yet. Plate-colonies on gelatine, for instance, develop quite differently, according to the condition of the gelatine; so that a feeble and slow-growing bacterium produces colonies quite unlike those developed by the same species when vigorous and quickly growing, not only owing to its peculiarities of growth as a feeble form, but also because the gelatine has altered during the intervening period.

5. The effect of changes of the environment on the growing organism itself is recognised as important.

6. With especial reference to the Thames bacteria, the past history of the organism isolated from the river implies causes of variation. The river water is a poor nutritive medium, and the organism is exposed to great changes of temperature, light, movement, &c., during its sojourn therein. Consequently the time it has been in the river affects the behaviour of the organism when isolated, just as we know that a bacterium is affected by the previous conditions of its culture in other media. Hence two colonies on a plate may look very different, and yet belong to the same species, one being developed from a cell that had been many days or weeks in the water, the other from one that had only been there a few hours. It may need weeks or months of cultivation under constant conditions to establish the identity of the two.

"On the Action exerted by certain Metals and other Substances on a Photographic Plate." By W. J. RUSSELL, Ph.D., F.R.S., Lecturer on Chemistry at St. Bartholomew's Hospital. Received May 13,—Read June 17, 1897.

Having some years ago prepared for the purpose of spectroscopic examination several uranium compounds, it was of interest to make further use of them by repeating some of the very important experiments which Becquerel has made with these compounds. He has shown that if the metal or some of its salts be placed on a photographic plate in perfect darkness, and allowed to remain there for some days, the plate becomes acted on, the action being rendered evident by the ordinary photographic process of development. This action is readily produced, and belongs apparently to all the salts of this metal, and, as Becquerel has shown, to uranous as well as uranic salts. It is very remarkable that this power belongs also to the salts when in solution, and, as the action passes through glass, solutions of the double chloride or of the nitrate contained in a thin glass bottle, when placed on a photographic plate, act readily upon it. While speaking of these compounds it may be well to record some experiments which have been made to determine whether they lost their peculiar activity on being kept in the dark.

On the 10th August last, specimens of yellow oxide, recrystallised nitrate, and chloride, the latter in solution, were each divided into two equal portions, and all placed in similar clean thin glass bottles. One sample of each was then placed in total darkness, and the other kept in the light. These samples have from time to time been tested by placing them on a photographic plate for a week and then developing the plate in the usual manner. Seven such examinations have been made at about one month's interval. No very marked difference between the samples in the light and the dark has occurred; on the whole the samples preserved in the dark have proved slightly the most active, and this was decidedly the case with all three specimens at the last examination on March 26. Another experiment was begun a little later with the black oxide of uranium, which appears to be one of the most active of the uranium compounds; equal weights of a sample of this body were placed in two similar pill-boxes with a glass bottom; one has been kept in the dark, and the other in the light; after five months there was no difference in the effect which they produced on the photographic plate (Plate 2). The experiments are being continued. When repeating these different uranium experiments and using a card painted with the yellow oxide, perforated zinc was made use of simply as a screen to show the

activity of the uranium compound by the density of the picture of the pattern formed, but in place of obtaining in all instances a negative of the perforated zinc, *i.e.*, the action occurring where the plate was exposed, and none where covered by the zinc, the reverse took place, and the greatest amount of action occurred underneath the zinc. This happened over and over again, and even when the experiment was varied in different ways, so that the only explanation of the action was that the zinc itself must be able to effect a change of the same kind as the uranium, at all events, to act on a photographic plate, and further experiment with zinc alone proved this to be the case; later on it became known to me that R. Colson had already described this action of zinc in a paper in the 'Comptes Rendus' in January last, and had also found that similar results could be obtained with cadmium and with magnesium. He explains this remarkable action as due to vapour given off by these metals.

Both before and after seeing the account of Colson's work a large number of experiments have been made with zinc under different conditions, and there is no doubt of the ease and certainty with which the results can be obtained. The zinc, as Colson states, must be bright; if well rubbed with coarse sand-paper it is most active: probably this may, to some extent, arise from increase of surface; if cleaned with acid or with caustic alkali it is not so active, and zinc in its ordinary condition after exposure to the air ceases to be active. The salts also have no power of acting in this way. A polished piece of zinc laid on a highly sensitive photographic plate will, under certain conditions even in four or five hours, so act on it that on development a complete picture of the zinc is produced, showing the scratches or any ruled lines or faint pattern drawn on it, or if flaws in the metal exist they are clearly seen. A slight pattern produced on zinc by pressing on it a piece of white net and then rubbing it down with fine emery or sand-paper will give a picture in which the pattern is very evident. In fact, such a pattern forms a satisfactory test of this action of the zinc. Very slight alterations of the surface are shown in the picture. Absolute contact of metal and plate is not necessary. If screens of different thicknesses of any inactive substance be interposed between plate and metal, thus preventing contact, the action still occurs; if the screen be very thin, a picture of the zinc surface is still obtained, but if thicker, only a dark cloudy patch is formed. Still further, if a thick piece of glass tubing an inch long be placed on a photographic plate, and the upper end covered with a piece of polished zinc, in a week to a fortnight distinct action will be found to have taken place below the zinc. Since the action then is not one of mere contact, the next point was to ascertain whether it would be transmitted

through different solid or liquid media. Glass, even of the thinnest kind, was found to stop the action, but many other substances allow of its transmission. For instance, the action takes place readily through celluloid, sheet gelatine gutta-percha tissue, collodion, vegetable parchment, real parchment, gold-beater's skin, tracing paper, and no doubt many other bodies. With all these bodies experiments have been made by placing the medium first in contact with the zinc and the photographic plate, then by introducing a screen so as to prevent the medium from touching the zinc, and then placing a second screen so that neither zinc nor plate were in contact with the medium. The screens were made of different materials, most commonly of either white cardboard or sheet indiarubber, and of different thicknesses. The details of each experiment need not be here described, but the general results obtained are that with thin sheet gelatine, either red, green, or blue when laid on the zinc, the action readily passes through, and a good clear picture of the surface of the zinc is obtained, and even with two sheets of gelatine a similar effect is produced. With thick sheet gelatine interposed the action on the plate still takes place, but of course the exposure must be longer. Warm solutions of gelatine were painted on polished zinc and allowed to harden; the action took place through such layers as readily as through the films. With screens used as before described to prevent contact the gelatine still allowed the action to take place through it. Thin sheets of celluloid about 0·28 mm. in thickness allowed the action to take place through them, and sheets 0·81 mm. in thickness also allowed the action to be transmitted. Again, gutta-percha tissue was found to act in the same kind of way as the gelatine and celluloid. The other media experimented with, although possibly not so uniform and continuous in structure as the foregoing, also allow this action to be transmitted to them.

Gold-beater's skin and tracing paper both allow the action readily to pass through, and pictures of the zinc are readily obtained. If either of these bodies be placed between a piece of perforated zinc and the plate, the perforations are very distinctly shown, or if they be placed between a double screen with corresponding holes cut, a picture of the holes is readily obtained.

Both vegetable and real parchment are also transparent to this action, but not so much so as the previously-mentioned substances; the vegetable parchment is more transparent than real parchment. When in contact with the zinc a picture of the zinc surface is obtained, but this is somewhat modified by the substance of the parchment.

If different kinds of ordinary papers, such as writing and drawing papers, be interposed between polished zinc and a photographic

plate, interesting results are obtained, for the pictures formed show clearly the structure of the papers, and also show that papers have very different powers of transmitting this action. Certain writing papers are quite opaque to the action; with others pictures of the structure and the watermark are easily obtained.

The painting a paper with India ink does not destroy its transparency. Obviously pictures of bodies, such as skeleton leaves or dried flowers, &c., are easily obtained in this way.

A mere difference of colour does not appear to alter the absorptive power of a medium; at least, this is the case with gelatine. The thin sheets of gelatine, whether red, green, or blue, have no difference in their absorptive power, and when gelatine, coloured with aniline dyes, is painted on polished zinc, the colour does not affect the amount of action which takes place. The same thing happens if demy paper be painted with different coloured solutions of gelatine. With ordinary pigments different results are obtained, but these results need not be discussed on the present occasion.

In addition to the metals cadmium and magnesium, mentioned by M. Colson, there are certainly many others which are able to produce effects similar to that produced by zinc. There are also certain alloys which can act in the same way. The following is a rough list of active metallic bodies approximately in the order of their activity: mercury, magnesium, cadmium, zinc, nickel, aluminium, pewter, fusible metal, lead, bismuth, tin, cobalt, antimony.

The above order, even if not absolutely correct, is sufficiently so to indicate that, although mercury is the most active, the other metals do not follow in the order of their fusibility or exactly according to any obvious physical property, but most nearly according to their position in the electrical series. Mercury is, then, at ordinary temperatures the most active metal, and its action is exercised not only when the photographic plate is placed half an inch or so above the metal, but when gelatine, gutta-percha, tracing paper, vegetable parchment, are interposed. It appears, however, that the action of the mercury does not take place as readily through gelatine, but more readily through gutta-percha than is the case with zinc.

Magnesium is also a very active metal, and very good pictures, showing every scratch on its surface, are easily obtained; also very marked effects are produced when both single and double screens are used. Cadmium also produces very good pictures, and is rather more active than zinc; nickel and aluminium are not quite so active, but give good pictures; then follow lead, bismuth, and tin. The last metal is by far the least active. Only a few alloys have at present been experimented with; brass gives no action, but ordinary pewter, and fusible metal, consisting of lead, bismuth, and tin, were found to have considerable activity, and are placed in the

list between aluminium and lead. That certain alloys should act in the same way as the metals is certainly of interest, and probably of considerable importance. The oxide and sulphate, both of zinc and cadmium, were found to be devoid of any power of acting on the photographic plate. Iron, gold, and platinum, are not active, and copper only very slightly. All the above results are founded on experiments in which the exposure lasted for one week; with longer exposure other metals will probably produce some action.

In order to determine whether moisture was an active agent either directly by affecting the medium or indirectly by affecting the photographic plate, experiments were made by exposing the plates under bell-jars, in which in one case there was water, and in the other sulphuric acid or calcium chloride, and even in these extreme cases no appreciable difference was found to occur, and even if the membrane was purposely damped it did not appear to aid the action except by bringing it closer to the metal, so that aqueous vapour is not apparently an active agent in producing these reactions. In an atmosphere of hydrogen the action takes place as it does in air. Carbon dioxide, under ordinary conditions, does produce an effect, but this probably arises from its action on the zinc plate. Alteration of temperature produces very marked effects; increase the temperature, and the action of the zinc is greatly increased; for instance, two similar plates, both wrapped in tinfoil and the plates separated from the zinc by means of a cardboard frame. One was placed on a water bath and exposed to a temperature of about 70° C., and the other placed in a vessel of ice at 0° C. After five hours the one which had been exposed to the high temperature had given a black picture, while the one at the low temperature gave a picture barely visible. A similar experiment was also made with nickel, and this gave, after heating to about 70°, a good dark picture, but the corresponding experiment, when the metal was kept at 0° for five hours, gave no picture at all. Aluminium, when treated in the same way, gave at the higher temperature only a faint picture, but at the lower temperature, even after two days, no picture at all. It has already been mentioned that this action of the metals cannot pass through even thin glass, nor can it pass through selenite, nor a layer of gum arabic, nor one of paraffin. Glass being impervious to the action, renders it somewhat difficult to try satisfactorily the action exerted by liquids, but celluloid may be used for this purpose; also mercury may be covered with a thin layer of water, and then its action entirely ceases. The action of certain salts in the dry state has, however, been tried by soaking non-glazed paper in different solutions, drying it, and then placing it, either with or without a screen, between the zinc and the photographic plate. These experiments have given some interesting results; for instance, paper

soaked in the following solutions, alum, potassium chromate, zinc sulphate, and quinine sulphate, renders the paper quite opaque to the action of the zinc.

No doubt this action of alum accounts for certain papers not allowing the action to pass through them. Some singular developments of this subject have arisen from experiments made while examining the metals. A piece of polished zinc was coated with copal varnish with the object of ascertaining whether the action would take place through such a medium, and in case it did, as it was thought at the time, of demonstrating that the action could not arise from metal vapour. The experiment was quite successful; the photographic plate, notwithstanding the varnish, was strongly acted on. The experiment was repeated several times, and always with the same result; but the pictures seemed rather too good, darker than those given by the zinc alone, and on trying the copal on plain glass instead of on zinc it proved that effects apparently similar to those obtained with zinc were produced. What is known as picture copal answers very well for these experiments. That prepared by Winsor & Newton has been used. This is painted or poured on a clean, warm glass plate, and allowed to harden completely. The plate can then be used in the same way as the zinc plates. If a photographic plate be laid on the hardened varnish for two to seven days, a picture of the varnish, showing the streaks it happens to have dried in, is produced. If screens be interposed so as to prevent contact between the copal and the plate, the action still occurs, and, in fact, readily passes down a tube 1 inch long. Therefore, as with the zinc, any figure cut out in an inactive screen is readily produced on the photographic plate. Substances which are transparent or opaque to the action of the metals seem to act in the same way towards copal. It is rather more active than zinc. Glass is perfectly impervious to its action, but celluloid, gutta-percha tissue, and gelatine it permeates more readily than zinc does. The activity of the copal varies considerably under different conditions. If the powdered gum be sprinkled on a glass plate and then fused, it is not so active as when picture varnish is used. If the solid gum be dissolved in pure alcohol and ether, and applied to a glass plate as before described, it is far more active than after fusion. Heating it in a water bath for a considerable length of time certainly deprives it of a considerable amount of its activity; but this can be revived by wetting it with ether and allowing it again to dry at ordinary temperatures. As with zinc, increase of temperature increases its activity to a great extent. Experiments similar to those with zinc were made with copal. A coated glass was exposed to a heat of abont 70°, and a similar one was kept at 0°. This one after five hours gave only a faint picture, whereas the heated one gave a dark picture, and a con-

siderable amount of action took place even through the cardboard screen. Many other bodies of the same nature as copal act in the same way. This has been proved to be the case with Damar and with Canada balsam, but copal seems to be the best representative of the class. Certain gums, such as gum arabic, gum senegal, have not the property of acting in this way. There are, however, a large number of bodies which have the power of acting in a manner similar to the copal; one of these is wood, and it possesses a very considerable amount of activity. Any ordinary smooth piece of wood laid on a photographic plate will act like zinc in impressing its picture on the plate.

A section of a young larch tree gave a good picture showing clearly the different rings and the layer of bark, which was the darkest part of the picture. The same section, when a film of gelatine was interposed between it and the plate, still gave a good picture. Wood which is thoroughly dried and hardened is also able to act in the same way.

A piece of mahogany 3·5 mm. thick, which had been in this form for at least thirty-five years and been carefully preserved in a dark cupboard, gave after a week's exposure a good picture, and the bottom of an old cigar-box acted equally well. Bodies such as straw, hay, bamboo, oiled silk, and, no doubt, many others, act in the same way. If wood, however, be painted with melted paraffin, it is no longer active. Ordinary charcoal also depicts itself on a photographic plate, but if it be heated for some hours in a covered crucible it loses this property. An ordinary piece of wood, if it be charred on one side by heating it with a Bunsen lamp, becomes remarkably active, as shown by placing it behind a screen with a pattern cut out. The action passes readily through different media, such as gelatine, tracing paper, &c., vegetable parchment, &c., and the structure of the charcoal is shown, when the action has taken place, even through a sheet of vegetable parchment. Coal and coke, sulphur, sugar, on the other hand, exert no action of this kind. When trying whether a copy of a lithographic picture could be obtained by placing behind it a plate of zinc, some curious results occurred. It would seem that printer's ink in most cases is not capable of acting, like copal, on a photographic plate, but that there are many cases in which it is a remarkably active substance. Specially so is the ink used in printing many of the newspapers. The 'Westminster Gazette,' for instance, is printed with an ink which very readily acts on a photographic plate. A portion of this paper with printing on only one side, laid with the blank side on the photographic plate, in a few days gives a remarkably black and distinct picture. If there be printing on both sides, then two pictures are obtained, the darker printing

becoming most evident on whichever side it may be. Interpose a sheet, for instance, of gold-beater's skin, and still the picture is obtained. The 'Standard' and 'Daily Graphic' are also very active, and the 'Times' only a little less so. The 'Evening News' is only slightly active, and the 'Morning Post,' 'Pall Mall,' 'Echo,' and 'Daily News' have not the property of acting in this way; at least, those copies experimented with had not. An admission ticket to the Society of Arts laid on a photographic plate, the ink away from the plate, also gave a very distinct picture.

Another singular case of an action of this kind was met with when experimenting with the uranium salts. Not having a sufficient number of small, clear glass bottles for a certain set of experiments, one of the compounds, the black oxide, was placed in a pill-box, believing that the action of the uranium would take place through the bottom of the box, and on developing the plate a dark circular space where it had stood was visible. The experiment was, therefore, considered very satisfactory, and, with different salts and for different objects, it was several times repeated. Ultimately it seemed evident that the uranium salts acted more strongly when in pill-boxes than in any other way, and on placing a pill-box without any uranium salt in it on a photographic plate it was found that action had occurred, as shown by the dark circular space produced.

The experiment was repeated over and over again, with the result that most pill-boxes have the power of acting on a photographic plate. Both new and old pill-boxes from different sources were experimented with, and almost all of them found to be active. There are, however, exceptions, and these, it was noticed, were always the more expensive and elaborate boxes. On examining the structure of a pill-box it was found that it is usually made of what is known as strawboard, covered with a thin white paper; on separately testing these two materials it was apparent that the white paper was without action on the plate, and that the strawboard was very active and produced exactly similar effects to those produced by the active pill-boxes. The inactive ones proved to be made of white cardboard, which is not an active substance. Samples of strawboard from several different sources have been tried, and all found to be active, and when separated from the photographic plate by means of screens, like the copal and the zinc, it gives a clear action. Different substances of a like nature have been tried, such as brown papers, &c. Some of them are more or less active, but none more so than common strawboard. Mr. Bevan was good enough to examine a piece of this active strawboard, but was unable to find any material other than straw present. Writing paper and, as mentioned before, white cardboard have not this power of acting on a photographic plate, but many kinds of brown paper

and no doubt many other bodies have the property. Many of the boxes in which photographic plates are packed are made of straw-board, but as the action does not pass through glass, the plates are but little or not at all acted on; but if a plate be laid face upwards in one of these boxes and left there for a week it will be very appreciably affected. If a small piece of glass be laid on the plate, it protects the film beneath, and shows clearly the amount of action which has occurred. If a box of this kind be painted inside with melted paraffin, this action does not take place. It happened that a few months before making the above experiments others were in progress in which black net was placed on a photographic plate simply to show clearly whether the plate had been acted on, and continually a reversed picture was obtained; this at the time could not be accounted for, but now the experiment was made of simply placing the black net on the photographic plate and leaving it there for some days; then on development a clear picture of the net was produced. The action is due to some material in the black dye, for white net does not act in the same way.

The action of the vapour from a few liquids on a sensitive plate has been tried. The plate was placed about half an inch above the liquid, and a screen, with holes cut in it, was fastened against the plate. Methylated spirit acted slightly on the plate; pure alcohol and ether had no action; benzene, coal-tar, crude wood spirit, linseed oil also, had no action, but turpentine and oil of cloves produced a slight amount of action.

Such, in outline, is an account of the experiments which have already been made on this subject. One point has led on to another, and some of the results were so unexpected that the experiments had to be repeated many times before full credence could be given to them. On the present occasion it is desired to do little more than record facts; further experiments, it is hoped, may lead to explanations not now evident. The supposition that all these active substances, the metals as well as organic bodies, give off a vapour capable of acting on a photographic plate, naturally suggests itself, and that copal does give off a vapour which directly or indirectly is active there can be no doubt. At the same time, it is at least difficult to suppose that the activity of such a body as strawboard should, after the treatment it has undergone, give off at ordinary temperatures sufficient vapour to produce the effects described, and the same applies to old dry wood, &c. Still more interest attaches to the action of the metals; do they emit a vapour so delicate in constitution and in such a quantity that it can readily permeate celluloid gelatine, &c., and produce a picture of the surface from whence it came, or is it a form of energy (possibly what has been called dark light) that these bodies emit? Zinc kept and polished in the dark

loses none of its activity. An experiment has been made with the object of reflecting the zinc action from glass. This did not succeed; whether this arose from the glass not being capable of effecting such a reflection, or whether a fortnight was not sufficient time to produce in this way a visible effect, is not known, but the experiment is being repeated. A photographic plate, suspended film upwards over a copal plate, was acted on round the edges in the way one would imagine a vapour to act. A similar experiment is being made over a zinc plate. The action of glass proves that there is at least a marked difference between the action exerted by metallic uranium and that by zinc and other metals.

It should be stated that it is only the most sensitive photographic plates which, without extremely long exposures, give the results described. The Mawson plate has generally been used in the foregoing experiments, but the Ilford special rapid plate acts equally well, and Edwards' isochromatic snap-shot plates are particularly sensitive to the action of the uranium salts. Lumière's extra rapid are not so sensitive as the Mawson and Ilford plates, and still less sensitive are the same firm's plates for yellow and green, and for red and yellow. Other sensitive plates have not been experimented with.

"On the Relative Behaviour of the H and K lines of the Spectrum of Calcium." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received May 27,—Read June 17, 1897.

[PLATE 4.]

The remarkable relative behaviour of the lines in the spectra of certain substances as they appear at and near the sun's limb, and in the atmospheres of stars of different classes, has long been before our minds as a problem of great interest, which there is reason to believe is capable of solution by the methods of the laboratory, and on which we have worked from time to time for many years. Without waiting for the results of other researches which are in progress, we think that it is desirable to put on record some definite results on the behaviour of the lines of calcium, which appear to us to be conclusive, and of great importance in forming a correct interpretation of many solar and stellar phenomena.

As early as 1872, Professor Young from a few weeks' work at Sherman on the spectra of the chromosphere and of the prominences, was able to point out that "the selection of lines seems most capricious; one is taken and another is left, though belonging to the same element, of equal intensity, and close beside the first." Especially he noticed that while the H and K lines of calcium are almost always

observable, the strong blue line as well as the other lines of this metal are very seldom seen. In his table of the chromospheric lines Professor Young gives for the frequency of this strong blue line the small number 3; while for the frequency of H and K, he gives respectively the high numbers 75 and 50.

From 1863, when I mapped the spectrum of calcium with a strong spark from metallic calcium* I have constantly used the lines of calcium as a comparison spectrum in stellar work. The experience was familiar to me that as the quantity of calcium salt on the electrodes became very small, H and K continued strong even when the other calcium lines had almost disappeared. The suggestion then occurred to me, that this behaviour of the lines might furnish a clue to the phenomena which take place near the sun's limb.

We were encouraged to use this experience as a guiding thought in the experiments about to be described, by the consideration that in the higher solar regions, where H and K appeared alone of the calcium lines, the density must be much less than at the lower level of the reversing layer. It seemed very probable that in the simple fact of difference of density, lay the true explanation of the modifications of the calcium spectrum as they are presented to us in solar and stellar phenomena.

The problem before us was, therefore, to find out by experiments in the laboratory, under what conditions the lines of calcium other than the lines H and K, and in particular the strong blue line at 4226·9, were so greatly enfeebled relatively to H and K, that they became quite insignificant, or even disappeared altogether from the spectrum, leaving the very simple spectrum of the two lines H and K, or nearly so.

Professor Lockyer states that:—"Some of the substances which have been investigated, including iron, calcium, and magnesium, have probably a definite spectrum, consisting of a few lines, which can only be completely produced at a temperature higher than any which is at present available in laboratory experiments." ('Roy. Soc. Proc.,' vol. 61, p. 205.)

In the case of calcium:—

"(4) A spectrum consisting of the two lines at 3706·18 and 3737·08 and the H and K lines, corresponding to a temperature higher than the average temperature of the spark, as before explained." (*Ibid.*, p. 161.)

Such a spectrum was not actually obtained, but experiments with a large intensity coil suggested that by a still greater increase of intensity of the spark such a simple spectrum might appear. The intensity of the strong blue line was reduced to one half of H and K. (*Ibid.*, Table, p. 162.)

* 'Phil. Trans.,' 1864, p. 139.

Kayser and Runge found 106 lines of the calcium spectrum to belong to the series of triplets; among the remaining lines they pointed out pairs with constant differences of wave-frequencies. Notably H and K, with a difference of wave-frequency of 222·9, and the more refrangible pair at 3737·08 and 3706·18, with a difference 223·1.

Messrs. Humphreys and Mohler in their experiments on the effect of pressure on the wave-lengths of metallic lines, found that in the case of calcium, the H and K lines were shifted only one-half as much as the blue line at 4226·9. We know far too little to justify us in forming any theoretical conclusions from this peculiarity of behaviour. Indeed there are no certain reasons why the lines of any substance should be equally shifted.

It is well known that calcium, in common with nearly all substances, gives a more complex spectrum under the conditions of the arc and spark than under those of a flame. Now in the Fraunhofer lines we have, as first shown by Kirchhoff and Bunsen, absorption spectra of the elements which correspond, speaking broadly, with those of the bright-lined spectra of the same substances as they are produced by the spark. In order, therefore, to study the modifications which the calcium undergoes in the higher regions of the chromosphere, in the prominences, and possibly in lower parts of the corona, as well as in the atmospheres of stars of different orders, it was clearly desirable that we should start with an ordinary spark spectrum. It was suggested to us strongly by the known rarer state of the gases in the regions above the photosphere, as well as by my long experience with the behaviour of calcium in comparison spectra that the modifications of the calcium spectrum which we were seeking, would be likely to show themselves under conditions of greatly reduced density of the calcium vapour.

Experiments.

For reasons which will be obvious later on, we elected to use throughout the experiments a spark of very small intensity.

1. The break of a 6-inch Apps coil was fixed at the position of smallest acting force of the spring. So much battery power only was employed as would be just sufficient to move the break. Under these conditions, when a jar was not in connection, the feeble spark would not pass when the distance between the points exceeded $1\frac{1}{4}$ inches.

2. In all the experiments a jar was intercalated.

3. The same length of exposure, a very short one of a second and a half, sufficient to bring out only the strongest lines of the spectrum, was used in each experiment.

4. Two sets of similar experiments were made; in one case with electrodes of platinum, and in the other with electrodes of iron. In the latter case the chief lines of iron were present with those of calcium.

Method adopted for reducing the Density of the Calcium Vapour.

(*a*) The spark was taken between electrodes of metallic calcium. It was assumed, as was confirmed by the appearance of the spark, that with metallic calcium for electrodes, the largest amount of calcium vapour would be present.

(*b*) The tips of the electrodes, iron or platinum, were slightly moistened with a strong solution of calcic chloride.

(*c*) The tips were slightly washed with pure water.

(*d*) The tips were again washed with pure water.

(*e*) The tips were then slightly moistened with a very weak solution, made by adding a drop of the strong solution to 2 ounces of water.

Our expectations were completely confirmed. Under the conditions (*a*) of greatest density of the calcium vapour, when metallic calcium was employed, the blue line was as strong and possessed the same diffuse character as H and K.

As the density of calcium was reduced, the lines were not found to be equally enfeebled, but, on the contrary, the blue line and the greater number of the lines were increasingly reduced in intensity relatively to H and K, until at last with the twice washed electrodes (*d*) the spectrum was simplified to the condition usually existing in the prominences, in which H and K only are present.

We now proceed to a more precise statement of the changes of relative intensity as they are presented in the photographs which accompany this paper.

Description of the Photographs on the Plate. (Plate 4.)

A. Photograph of the spark when both electrodes consist of metallic calcium. Here we have present doubtless the largest amount and greatest density of calcium vapour. The winged character of H and K, of the blue line, and of the pair more refrangible than H and K, is well seen, showing that this appearance comes out when the gas is dense. If the greater extension of the wings of H is allowed for, and the line H carefully distinguished from the fine lines close to it, it will be seen to possess very nearly the same strength, both as regards width and length, as the blue line at 4226·9. The strength of this blue line under this condition of density is about the same as that of the line at 3737, and rather greater than the line beyond at 3706.

B. Spark taken with one electrode only of metallic calcium, the other electrode being of platinum. In this case the effect of a smaller density of the calcium vapour is clearly shown in the greatly reduced wingedness of the lines. It will be remarked that the diminished density has had the greatest influence on the pair at 3737 and 3706; these lines are now much less strong than the blue line, which still holds its own, and remains about as strong as H and K. The lines of the more refrangible pair are no longer diffuse at the edges.

C. Spark taken between platinum electrodes moistened with a strong solution of calcium chloride. Here the effect of a smaller quantity of vapour begins to tell strongly upon the intensity of the blue line relatively to H and K. It may now be estimated at less than one-fourth of the intensity of H. At the same time, H and K have almost completely lost their diffuse character, and have become thinner and more defined.

D. The electrodes as left in the former experiment were slightly washed with pure water, leaving a trace only of calcium chloride. There is, as might be expected, an advance in the enfeeblement of the blue line and of the more refrangible pair, relatively to H and K.

E. The electrodes were again slightly washed with pure water, so that a still smaller trace of calcium chloride must have remained upon them. The enfeeblement of the blue line and of the pair has now become very great, while H and K, though thinner, remain strong.

F. The electrodes were once more washed with pure water, reducing still further the trace of calcium chloride which remained upon the platinum wires. The blue line has now practically disappeared, and the refrangible pair become very thin. The H and K lines have become thin and defined, as they usually present themselves in the prominences.

G. The electrodes remaining as they were left after the last experiment (F), the spark was taken upon a background consisting of a faint solar spectrum. The blue line has now completely disappeared, leaving H and K strong.

H. Once more the electrodes were washed, with the expectation of having removed completely the last remaining trace of calcium. To our surprise, when the photograph was developed, the lines H and K came out alone. The more refrangible pair had now faded out as well as the blue line. H and K were now thin, and extended but a short distance in the spectrum.

It must be remembered that the only condition which was varied during this set of experiments was the amount or density of the calcium vapour. The changes of relative intensity, and the modifications of the calcium spectrum produced thereby as shown in the

succession of photographs on the plate, correspond closely to the behaviour of calcium at different levels near the sun's limb, and in the atmospheres of stars of different orders. There can remain little doubt that the true interpretation of the changes in appearance of the calcium lines in the celestial bodies is to be found in the different states of density of the celestial gases from which the lines are emitted or by which they are absorbed.

A similar set of experiments was made with iron electrodes. Precisely similar results as to the relative enfeeblement of the lines, as with calcium chloride on platinum electrodes, were obtained. Of course the iron lines were also present. As might be anticipated, in consequence of the simultaneous presence of the iron vapour, the lines of calcium were thinner than when platinum was used.

Outside the range of wave-lengths which could be conveniently given on the plate, far on in the ultra-violet, there is a pair of strong lines which behave very much as H and K. It remains visible in photograph H, when the pair at 3737 and 3706 have disappeared. This pair is situated at 3158·98 and 3179·45.

It is desirable to point out again that all the photographs on the plate and the far ultra-violet lines, were obtained with a spark of quite unusually small intensity, which was purposely made as little hot as possible, in order to emphasize the important fact that the determining condition of the spectral changes under discussion is not one of increase of temperature.

In the modifications of the calcium spectrum arising from variations in the relative intensities of the lines which have been discussed in this paper, and which correspond to those observed in the celestial bodies, there does not appear to us any reason for assuming, much less any direct evidence in favour of, a true dissociation of calcium, that is, of its resolution into chemically different kinds of matter.

It would be remarkable if, by decomposition through increase of temperature, a large number of lines of a spectrum should become relatively enfeebled, and that as the result of decomposition a spectrum should become simpler, and not as analogy would suggest, more complex.

It is of importance to keep in mind that the recent chemical use of the word *dissociation* is not equivalent to true decomposition, *i.e.*, to a resolution of the original substance into two or more chemically different kinds of matter. It may, and does often mean not more than a different arrangement of the parts of the molecule, while those parts are all chemically matter of the same kind as the original molecule. As in the case of the resolution of a compound molecule of peroxide of nitrogen into two identical half molecules; or, in the separation of a molecule of elementary iodine into two half molecules or atoms of identical chemical

characters. Such dissociations are well known, and are not of infrequent occurrence, and may, indeed, take place in connection with some of the spectral changes of a substance observed under different conditions. On the other hand, a true decomposition of a chemical element, that is, a breaking up of the molecule into simpler and quite other kinds of matter, though a notion familiar to chemists since Prout's time, and regarded as theoretically possible, is, as yet, unknown as a matter of fact.

Conclusions.

These experiments seem to us to furnish an adequate and consistent explanation of the behaviour of the calcium lines at and near the sun's limb. Near the photosphere where the absorption mainly takes place, by which the dark lines of the solar spectrum are formed, there would be, we should expect, a much greater density of calcium vapour than at a higher level, and we find the Fraunhofer line at 4226·9 strong but much less broad than H and K. The recent photograph of the reversing layer shows that the broad shading of H and K is not produced there, but probably, as Prof. Jewell concludes from his measures, lower down where the gas is still denser, which is in agreement with photograph A on the plate.

Higher up in the chromosphere, in the prominences, and possibly in the lower coronal regions, the decrease of the density of the gases composing them must be rapid, and the temperature gradient as determined by expansion must be also rapid. We have clearly to do, in these regions, with calcium vapour in a rarer state, and except so far as the molecules may have carried up within themselves to some extent the higher heat of a lower level, or through imperfect transparency, the gases may have received heat from the sun's radiation, it must be at a much lower temperature than near the photosphere. Now, the changes of the calcium spectrum which take place in these regions, are those which correspond in our experiments to a very small amount of calcium vapour, and a spark of small intensity.

On account of the violent commotion which must exist through the strong convection currents at the sun's limb, we should not be surprised to find some calcium vapour, notwithstanding its greater density, carried high up together with the lighter substances such as hydrogen and helium. Our experiments show how strongly the H and K lines may come out when a trace only of calcium vapour is present, and so, it seems to us, offer a possible explanation of the great height at which these lines may be sometimes recognised. At no very great distance from the surface of the sun the gases must become too tenuous to give a visible spectrum; but it may well be that the brilliant radiations of even very rare calcium gas at H and K

may show in our instruments for some distance after the hydrogen and the other light matter associated with it, have become too subtle to furnish a spectrum that we can detect.

The relative behaviour of the lines of the calcium spectrum as they present themselves in the different orders of stellar spectra when interpreted by the terrestrial experiments described in this paper, will throw much light on many of the important questions which are still pending in celestial physics. In forming conclusions as to the state of the stellar atmospheres from the different densities which may be indicated by the modifications of the calcium spectrum, it must be borne in mind that, as I have said elsewhere:—

"The conditions of the radiating photosphere and those of the gases above it, on which the character of the spectrum of the star depends, will be determined not alone by temperature, but also by the force of gravity in these regions; this force will be fixed by the star's mass and its stage of condensation, and will become greater as the star continues to condense."*

It may be, though on this point we have as yet no sufficient data, that though the stars are built up of matter essentially similar to that of the sun, the proportion of the different elements is not the same in stars which have condensed in parts of the heavens widely distant from each other, or at epochs greatly separated in time.

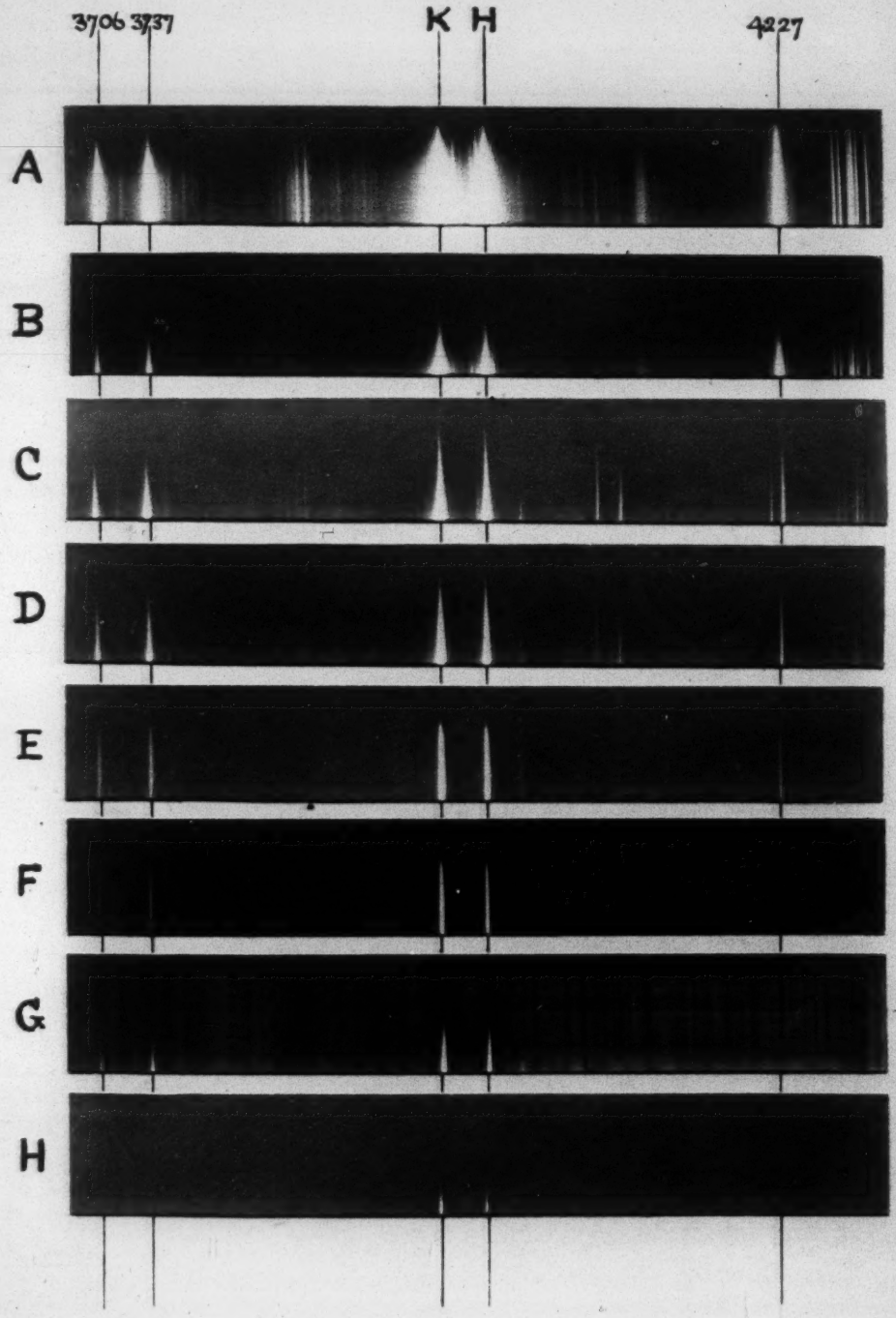
It does not seem desirable to discuss any of these questions at the present time, as we hope before long to offer some explanation of the, to some extent analogous, relative behaviour of the lines of some other substances as observed in the sun and stars.

[The following letter from our friend Professor Liveing, which he has given us permission to publish, contains an account of early experiments on the spectrum of calcium which not only support, by a different method of working, the conclusions of our paper, but also seem to suggest the possible occurrence of the line H without the line K. In our experiments both lines were always present, the line K being longer and stronger than H; conditions of the calcium lines which are in agreement with the photographs of the prominences taken by Hale and by Deslandres.

"I have been looking up some observations of Dewar's and mine on the H and K lines of calcium made in 1879. We found that when we used, for the arc, carbon poles which had been heated for two days in chlorine to remove metals, the calcium lines were not at first visible in the arc, but after a time H was seen alone and not strong; after a further time, K was seen, and then other calcium lines came out. No doubt the calcium had been pretty well removed from the carbon rods to some depth, but not entirely from the

* Address, 'Brit. Assoc. Report,' 1891, p. 15.

SPARK SPECTRA SHEWING EFFECT OF DENSITY ON THE RELATIVE INTENSITIES OF THE LINES OF CALCIUM.



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interior, so that as the carbon burnt away in the arc the calcium in the interior became manifest.

"Again, we found that when we used a perforated pole and passed a stream of hydrogen into the arc through it, H and K could be both entirely obliterated; but by then reducing the current of gas they gradually reappeared, and H always came out first and afterwards K; and H remained stronger than K, until they had both resumed their ordinary appearance. This observation was repeated several times.

"Both sets of observations, those with the purified carbon poles and those with the perforated pole, seem to me to confirm your conclusions.

"In the case of those with the perforated pole, the stream of hydrogen diluted the calcium vapour, and the degree of dilution was controlled by the rate at which the gas was introduced. The mass of gas passing was too small to reduce the temperature by any considerable amount, or even, I should think, to any sensible amount.

"We found also that metallic lithium, introduced into the arc, produced effects similar to those produced by hydrogen, that is, that it reduced very much the strength of the H and K lines. If more than a very minute piece of lithium were introduced, the arc was invariably broken, so that we did not notice the complete obliteration of H and K with the lithium.

"The reduction of the strength of H and K, in this case, I attribute to the dilution of the calcium vapour by that of lithium." —June 25.]

"Further Observations of Enhanced Lines." By J. NORMAN LOCKYER, C.B., F.R.S. Received May 12,—Read June 17, 1897.

In a recent paper I gave an account of a series of experiments having for their object the determination of the lines which were enhanced in the spectra of iron, magnesium, and calcium, on passing from the arc to a high temperature spark, and I pointed out the presence of these lines in the spectra of the hotter stars and in the solar chromosphere.

The spectra of the following additional elements have since been investigated in a similar manner, and the enhanced lines have been tabulated and compared with chromospheric and stellar spectra.

List of Additional Elements examined for Enhanced Lines.

Aluminium.	Lead.	Sulphur.
Antimony.	Lithium.	Tantalum.
Barium.	Manganese.	Tellurium.
Beryllium.	Molybdenum.	Thorium.
Bismuth.	Nickel.	Titanium.
Carbon.	Niobium.	Tin.
Cadmium.	Palladium	Tungsten.
Cerium.	Potassium.	Uranium.
Chromium.	Rhodium.	Vanadium.
Cobalt.	Silicium.	Yttrium (and Erbium).
Copper.	Silver.	
Erbium (and Yttrium).	Sodium.	Zinc.
	Strontium.	Zirconium.
Lanthanum.		

The list of lines is not given here for the reason that so far the series of comparisons with a Rowland grating of 21 ft. radius is not complete. Nevertheless the approximate wave-lengths already determined are of the same order of accuracy as those of the chromospheric and stellar lines, and the supposed coincidences depend upon the occurrence of several lines of a given substance rather than upon an absolute agreement in the deduced positions of any single line.

In the former paper I drew attention to the fact that while the enhanced lines of magnesium (4481·3) and calcium (H and K) remained feebly visible in the hottest stars included in the discussion, the enhanced lines of iron vanished entirely in such stars, and were thinner than those of magnesium and calcium in stars which are approaching the highest temperature as determined by the length of the spectrum in the ultra-violet. Similar differences of behaviour have also been noted among the substances which have since been investigated.

These differences in the case of metals of the iron and calcium groups are shown in the following table, in which the stars are arranged in the order of temperature previously deduced from the discussion of the lines of iron, magnesium, and calcium, and the name of each substance considered is placed opposite that of the hottest star in which its enhanced lines remain visible.

It thus appears that while calcium remains visible up to the highest temperatures, the enhanced lines of strontium probably cease to be visible at temperatures higher than that of γ Cygni, while those of barium have not yet been certainly traced in any of the stars. The order of appearance of the metals of the calcium group thus conforms with the chemical order. Of the metals of the iron group,

Name of Star.	Calcium group.	Iron group.
Bellatrix	Ca	
ζ Tauri	—	Fe, Mn.
Rigel	—	Co, Ni.
η Leonis	—	
α Cygni	—	
γ Cygni	Sr	
α Tauri	—	
α Orionis	[Enhanced lines of barium probably do not appear]	

iron and manganese show their enhanced lines at temperatures as high as ζ Tauri, while cobalt and nickel do not extend higher than Rigel. Among the substances recently examined, the case of silicium is of special interest. The enhanced lines of this substance, two of which have wave-lengths about 4128·6 and 4131·4, are amongst the most conspicuous lines in α Cygni and remain distinctly visible even in Bellatrix, after the lines of iron, manganese, nickel, and cobalt have vanished. In its stellar behaviour then, this substance resembles Ca and Mg.

So far as the work has gone, the comparison of the enhanced lines with the spectrum of the chromosphere reveals several facts of importance. In the case of iron, I have already pointed out* that the enhanced lines were all present in the chromosphere during the eclipse of 1896, and most of them in that of 1893, their intensities being greater than those of the corresponding Fraunhofer lines. Many of the characteristic arc lines of iron also appear in the chromosphere, but the presence of the enhanced lines with such great intensities indicates that at least in some parts of the chromosphere the temperature of the iron vapour is considerably higher than that of the iron vapour which is most effective in producing the Fraunhofer lines. A similar result is obtained when other substances are considered. The special importance of the enhanced lines in the chromosphere is shown by the following figures relating to substances which have been most completely studied:—

No. of enhanced lines of Fe, Mg, Ca, Mn, Ni, Co, Ti, so far tabulated in the region F to K	63
No. of these lines photographed in eclipse of 1893	28
Percentage of enhanced lines of Fe, &c., in eclipse lines of 1893.....	44
No. of enhanced lines photographed in eclipse of 1896	41
Percentage of enhanced lines of Fe, &c., in eclipse of 1896..	65

* 'Roy. Soc. Proc.,' vol. 60, p. 475.

These numbers show that the chromospheric spectrum is largely composed of enhanced metallic lines in addition to the lines of hydrogen and the cleveite gases.

In the Fraunhofer spectrum enhanced lines may be regarded as wanting, for in the case of iron and magnesium, at least, they only appear with the feeble intensities which they have in the arc spectrum, while the characteristic arc lines are strong. Here then we find the cause of the dissimilarity of the chromospheric and Fraunhofer spectrum which is indicated by the following figures:—

No. of Fraunhofer lines tabulated by Rowland in the region F to K.....	5694
No. of lines photographed in the same region, eclipse 1893	164
Percentage of Fraunhofer lines	3
No. of lines photographed in the same region, eclipse 1896.....	464
Percentage of Fraunhofer lines	8

Clearly then, the chromosphere as photographed in the eclipses of 1893 and 1896, is a region of high temperature in which there is a corresponding simplification of spectrum as compared with the cooler region in which the Fraunhofer absorption is produced.

"The Total Solar Eclipse of August 9, 1896. Report on the Expedition to Kiö Island." By J. NORMAN LOCKYER, C.B., F.R.S. Received May 15,—Read June 17, 1897.

(Abstract.)

The observing party consisted of Mr. Fowler, Dr. W. J. Lockyer, and myself, and the selection of Kiö Island as an observing station was rendered possible by the grant of the services of H.M.S. "Volage." Although the expedition failed in its main objects, because of unfavourable weather, it is considered desirable to put on record an account of the arrangements which had been made to secure observations, more especially as a new feature was introduced in the training of a large number of officers and men to take part in the observations.

Profiting by the experience gained in previous eclipses, prismatic cameras of the highest available powers were taken out, as well as a powerful integrating spectroscope. To supplement the work of these photographic instruments, a number of prisms and small slit spectroscopes were provided for use by such assistants as were available. The voluntary services of 74 officers and men of H.M.S.

"Volage" made it possible to extend the original programme so as to include records of all the attendant phenomena.

The paper gives an account of the preparation of instruments and huts, and of the organisation of the "Volage" observers into parties for different branches of the work. For the benefit of others who may be similarly circumstanced on future occasions, full particulars of these working parties and the instructions issued to them are included in the paper, and the arrangements for working the larger instruments are also described.

On the morning of the eclipse the sky was almost entirely overcast, and the sun was quite invisible during totality.

No photographs were obtained, but some observations of temperature, colours of the landscape, and the general phenomena of totality were secured. As shown by two thermometers screened from the direct rays of the sun, the temperature fell 0·9° F. from first contact to totality, and rose the same amount between totality and last contact. A fully exposed thermometer at another place indicated a fall of 6·5° F., and a subsequent rise of 1·5° F. during the same intervals.

"On the Classification of Stars of the δ Cephei Class." By J. Norman Lockyer, C.B., F.R.S. Received May 17,—Read June 17, 1897.

Introduction.

The spectrum of δ Cephei is one of a group with special characteristics. While containing a great number of fine metallic lines, giving it more or less the same general appearance as the solar spectrum, it shows many lines which are either faint in the solar spectrum or are altogether absent. In a former paper* I showed that the spectrum is practically identical with that of γ Cygni, which my previous work had indicated to be a star of increasing temperature.†

The chief argument which I had employed in favour of placing γ Cygni on the ascending side of the temperature curve was based on the presence of certain special lines, which occur with increased importance, in the spectrum of α Cygni, which differs very widely from the solar spectrum, and has a close relationship to the Orion stars.

Further, the association of a special kind of variability with some of the stars having a spectrum of this type seemed to strengthen the view that the constitution of such stars must be vastly different from that of the sun. Dr. Vogel, however, has classified two stars of the same group as δ Cephei, namely, η Aquilæ and 10 Sagittæ,

* 'Roy. Soc. Proc.,' vol. 59, p. 103.

† 'Phil. Trans.,' A, vol. 184, p. 718.

with the sun in his Class II*a*, and more recently Dr. Scheiner has placed another star of the group, α Persei, between α Cygni and the sun. It is right to add that Drs. Vogel and Scheiner differ very considerably as to the classification of α Cygni, and this still further complicates matters.

It will be seen that the question between the two classifications is a very sharp one. Is the difference between stars like δ Cephei and stars resembling the sun solely due to a temperature difference, as it is on Vogel's view, or does it in part represent, as I contend, a physical difference between the two classes, since on my view stars like δ Cephei consist of uncondensed swarms of meteorites of increasing temperature, while those like the sun are masses of vapour in which there are photospheres and relatively quiet atmospheres?

Since the work recently communicated to the Royal Society enables me to go still further forward, I propose to discuss in the present paper the points relating to the classification of stars of the δ Cephei class.

In Vogel's classification all stars are regarded as cooling bodies, while one of the chief points of mine is the distinction between stars which are getting hotter and those which are becoming cooler. Thus, while stars like δ Cephei and those like the sun are grouped together by Vogel in his Class II*a*, they are in mine divided into two groups, Group III including δ Cephei and Group V the sun.

When I first suggested the new classification I fully recognised the difficulty of separating Groups III and V. Thus I wrote in 1888:—

"With our present knowledge, it is very difficult to separate those stars the grouping of which is determined by line absorption into Groups III and V, for the reason that so far, seeing that only one line of temperature, and that a descending one, has been considered, no efforts have been made to establish the necessary criteria."*

In the following year I gave the results of some visual observations of stellar spectra which seemed to justify the separation of the stars with line spectra into two groups, and to suggest the necessary criteria for distinguishing them.†

Soon after this a photographic investigation of stellar spectra was commenced at Kensington, and as a result of that work the conclusions at which I had arrived from a discussion of the eye observations were confirmed. The additional details shown by the photographs enabled me to extend the classification given in 1888.‡

* 'Roy. Soc. Proc.,' vol. 44, p. 27.

† 'Roy. Soc. Proc.,' vol. 45, p. 380.

‡ 'Phil. Trans.,' A, vol. 184, p. 125 (1893).

Stars resembling γ Cygni formed the sub-group IIIβ.

In November, 1895, I gave an account* of the spectra of five short period variable stars, δ Cephei, η Aquilæ, ζ Geminorum, T Vulpeculæ, and S Sagittæ, and pointed out that these belonged to the same sub-group as γ Cygni, which, however, is not a variable star; α Ursæ Minoris has also a spectrum which has since been recognised to be identical with that of γ Cygni and δ Cephei, while α Persei differs so slightly from them that it may also be classified with them.

These I now group together, and for convenience of reference I designate them the δ Cephei class.

Dr. Vogel's Classification.

Only two of the stars which have been mentioned as belonging to the δ Cephei class, namely, η Aquilæ and S(10) Sagittæ, were included in Vogel's spectroscopic *Durchmusterung*, published in 1883, and both are classed without further comment as stars of Class II*a*.† This type of spectrum was thus defined: "Spectra with very numerous metallic lines, which are easily known by their intensity, especially in the yellow and green. The hydrogen lines are for the most part strong, but are never so broad as in the case of Class I*a*. In some stars the lines of hydrogen are faint, and in these faint bands can be generally recognised in the less refrangible portion of the spectrum."

So far as Vogel's classification serves as a guide, then, all the stars of the δ Cephei class would be classed with the sun.

Dr. Scheiner has discussed one of the stars of the δ Cephei class, α Persei, in some detail.‡ Attention is specially drawn by him to the differences between the spectrum of α Persei and that of the sun.

In the following table I have brought together the lines of α Persei which Dr. Scheiner states to be more intense than in the sun, and have compared them with δ Cephei and α Cygni as photographed at Kensington. The remarks in the last column are those made by Dr. Scheiner with regard to the spectrum of α Persei.

* 'Roy. Soc. Proc.,' vol. 59, p. 103.

† 'Public. Astr. Obs. zu Potsdam,' vol. 3, p. 200.

‡ 'Public. Astr. Obs. zu Potsdam,' vol. 7, Part II, p. 329.

Special Lines in α Persei.

α Persei (Scheiner), λ Potsdam.	δ Cephei (Lockyer), λ Rowland. Maximum intensity = 10.	α Cygni (Lockyer), λ Rowland Maximum intensity = 10.	Remarks on α Persei (Scheiner).
4290·2	4290·7 (8)	4290·7 (4)	Stronger than in sun.
4306·3	4306·4 (5)	?	" "
4310·1	..	..	No corresponding solar line.
4313·4	4313·3 (4)	4313·3	Stronger than in sun.
4321·4	?	4321·3 (2)	Much stronger than in sun.
4331·0	4331·1 (6)	4331·1 (2)	Stronger than in sun.
4344·7	4344·5 (5)	4344·5 (2)	" "
4375·2	4375·0 (10)	4375·0 (2)	Much stronger than in sun.
4387·2	?	..	Stronger than in sun.
4391·4	4391·4 (5)	..	" "
4394·4	4394·4 (6)	..	" "
4400·0—4400·9	Broad line (8)	4400·0 (4)	" "
4411·1	4411·0 (4)	..	No solar line.
4413·6	4413·6 (1)	..	" "
4416·9	..	4417·4 (7)	" "
4450·7	4451·1 (5)	4451·1 (4)	Stronger than in sun.
4461·7	4462·0 (5)	..	" "
4464·7	4465·0 (2)	..	" "
4468·7	4468·4 (6)	4468·4 (5)	" "
4471·1	4471·1 (3)	4471·0 (2)	No solar line.
4473·1	4473·1 (3)	4473·1 (2)	Stronger than in sun.
4481·6	4481·3 (7)	4481·3 (9)	" "
4488·6	4489·3 (5)	4489·3 (4)	" "
4491·8	4492·0 (3)	4492·0 (4)	" "
4501·5	4501·5 (7)	4501·5 (4)	Much stronger than in sun.
4508·5	4508·5 (5)	4508·5 (6)	" "
4515·6	4515·6 (6)	4515·6 (6)	Stronger than in sun.
4520·6	4520·5 (3)	4520·5 (6)	" "
4534·4	4534·3 (7)	4534·3 (6)	Much stronger than in sun.
4545·3	4545·3 (6)	4545·5 (1)	" "
4549·9	4549·9 (7)	4549·9 (8)	Stronger than in sun.
4564·1	4564·1 (4)	4564·1 (5)	" "
4572·0	4572·0 (5)	4572·0 (5)	" "

Dr. Scheiner also remarks on a few of the special lines of α Ursæ Minoris, another member of the δ Cephei class. My own photographs show almost absolute identity with δ Cephei, so that a special discussion of this star would be superfluous.

It will be seen that the special lines of α Persei are nearly all present in δ Cephei and α Cygni, and that they are all either faint in, or absent from, the solar spectrum.

The spectrum of δ Cephei has been investigated by Belopolsky,* and he gives a table showing that it differs in many respects from that of the sun. Attention is specially drawn by him to some of the

* 'Ast. Nach.,' No. 3338, p. 19.

lines which are strongly marked in the spectrum of δ Cephei as compared with corresponding lines in the solar spectrum photographed with the same instrument. A discussion of these differences shows that the more special lines of δ Cephei, like those of α Persei, are very prominent in α Cygni.

In spite of these differences, Dr. Scheiner classes α Persei with stars like the sun, but regards it as a transition stage between α Cygni and the sun. He says: "From the general conclusions, the above list gives us quite a curious and important result. If one compares these lines with those in the spectrum of α Cygni, which is of special interest as an advanced spectrum of Class I*b*, one finds that, out of sixty lines in α Cygni, twenty-one occur in the list, and all of these in α Persei. The fact, therefore, remains that nearly half the lines which in the spectrum of α Persei show divergences when compared with the solar spectrum appear certainly in any case in the spectrum of α Cygni and determine its peculiarity.

"Now α Cygni belongs to Class I*b*, α Persei to Class II*a*, and the plausible conclusion appears to me, therefore, that α Persei had previously a spectrum similar to that of α Cygni, and that in this case we have the missing connexion between I*b* to II*a*." *

The spectrum of α Tauri, which, as I shall show, must be regarded as a condensing swarm, closely resembles that of the sun; so that Dr. Scheiner's evidence would equally place α Persei intermediate between α Tauri and α Cygni.

Difficulties connected with Vogel's Classification.

The close association of stars like δ Cephei with those like α Cygni makes it here important to discuss the place of α Cygni in Vogel's classification. With regard to this star Dr. Scheiner writes: "The spectrum of α Cygni, in spite of the large number of its lines, has no resemblance with that of the sun. While it is possible to identify most of the lines with solar lines in respect of their position, yet the total lack of agreement as to intensity of the lines makes many of these identifications worthless." †

Dr. Scheiner has classified α Cygni in Vogel's Class I*b*, which was thus defined in Vogel's original classification: "Spectra in which the metallic lines are few in number, and very faint or entirely imperceptible, and in which the hydrogen lines are lacking."

In view of the photographic results obtained at Potsdam, the last clause in this definition was corrected in 1888, so that it reads: "and the strong hydrogen lines of Type I*a* are lacking." ‡ This has again been modified by Dr. Scheiner, and the characteristics of

* 'Potsdam Observations,' vol. 7, Part II, p. 331.

† Scheiner's 'Astronomical Spectroscopy' (Frost's translation), p. 247.

‡ 'Ast. Nach.,' vol. 119, p. 97.

Type I*b* are stated as "spectra in which the hydrogen lines and the few metallic lines all appear to be of equal breadth and of sharp definition."* It is on the ground of this greatly modified definition that α Cygni is included with stars like Rigel in Class I*b*.

Dr. Vogel, however, is not prepared to accept Dr. Scheiner's amended definition of Class I*b*. He writes: "However justifiable it may be to regard the peculiarly sharp spectral lines of the stars above mentioned and a few others of the same kind as worthy of special consideration, the adoption of this proposal would make it necessary to separate a number of stars (including those of Orion) whose relationship is placed beyond all question by the investigations I have referred to, and to place them with α Cygni, which has a materially different spectrum." †

In the same paper Dr. Vogel brings forward a new definition of his Class I*b* in the following terms: "Spectra in which, besides the still dominant hydrogen lines, the lines of cleveite gas appear, and above all the lines λ 4026, λ 4472, λ 5016, and λ 5876(D_3). The lines of calcium, magnesium, sodium, and iron are also more or less numerous in spectra of this subdivision."

This new definition excludes α Cygni from Class I*b* in Dr. Vogel's opinion, and he places this star in Class I*a* 3 of his extended classification of spectra of the first class. This sub-class is thus defined: "Spectra in which the calcium line λ 3934 has nearly the same intensity as the hydrogen lines. In occasional instances it is still sharply defined at the edges, or it may be broader and more intense than the hydrogen lines, and very diffuse, forming with the hydrogen line $H\epsilon$(λ 3970), which is greatly intensified and broadened by the calcium line λ 3969, a conspicuous pair.

"In the spectra of this division the lines of the cleveite gas cannot be recognised; on the other hand, numerous strong lines of different metals, particularly lines of iron, are always present. The lines of hydrogen are still always dominant. H_δ is plainly apparent among the other lines, and the group G is less conspicuous than H_γ. This subdivision forms the direct connecting link with the spectral class II, in which the hydrogen lines no longer play a prominent part in comparison with the lines of other metals."‡

According to this definition, α Cygni is classed with Procyon, a star which in the main resembles the sun, and this notwithstanding Scheiner's remark that the spectrum of α Cygni bears no resemblance to that of the sun.

It is clear, then, that α Cygni cannot be classified satisfactorily on the supposition that all stars are cooling.

* Scheiner's 'Astronomical Spectroscopy' (Frost's translation), p. 245.

† 'Astrophysical Journal' (1895), vol. 2, p. 343.

‡ 'Astrophysical Journal' (1895), vol. 2, p. 344.

Reference to the Old Evidence of Two Series.

In previous communications to the Royal Society I have stated the evidence which shows that in stars of Group II, such as α Orionis, the occurrence of radiating carbon vapour is an indication that these stars, like comets, consist of uncondensed swarms of meteorites. Since the temperature of a condensing swarm of meteorites must be increasing, in accordance with thermodynamical principles, stars like α Orionis must be placed on the ascending arm of the temperature curve. The photographs show that the spectrum of stars like α Tauri is almost identical with that of α Orionis so far as the lines are concerned, and since one of the flutings in the red in the spectrum of α Orionis also appears in α Tauri, this star must also be regarded as one of increasing temperature.

The discussion of the Kensington photographs led me to place γ Cygni next to α Tauri in the series of stars with increasing temperature,* and we now know that δ Cephei must be classed with this star. I pointed out that the spectrum of γ Cygni "has much in common with that of α Tauri, but there is less continuous absorption, and many of the lines of α Tauri thin out. The next step to α Cygni is rather a long one, but it seems very probable that if more photographs were available intermediate spectra would be found. It will be seen, however, that in α Cygni the hydrogen lines are intensified as compared with γ Cygni, and that all the important lines of α Cygni agree in position with prominent lines in γ Cygni. . . . . In passing to Rigel the more important lines of α Cygni are retained, and a few new lines make their appearance."

My argument was, then, that with an increase of temperature a star like α Tauri would develop into one like δ Cephei, which, with further increase, would pass through successive stages represented by α Cygni, Rigel, and Bellatrix. To justify this it is accordingly necessary to show greater reason for associating δ Cephei with α Tauri than with a star like the sun, which we know on other grounds to be cooling.

From the great similarity of the line spectra of α Tauri and the sun, it is clear that the argument will not entirely depend upon the identity of lines in the spectra of α Tauri and δ Cephei, but upon general and specific differences between δ Cephei and the sun.

Evidence from Enhanced Lines.

In a recent communication to the Royal Society,† I have shown that the investigation of the lines enhanced in the spark spectra of

* 'Phil. Trans.' (1893), A, vol. 184, p. 708.

† 'Roy. Soc. Proc.,' vol. 61, p. 181.

metals has enabled me to apply new criteria in the classification of stellar spectra.

A study of these lines shows at once that δ Cephei is hotter than either α Tauri or the sun, and that the difference between its spectrum and that of α Tauri or the sun is certainly in part due to this difference of temperature of the absorbing vapours. The lines which are stronger in δ Cephei than in the sun include many of those which have been found to be enhanced in the spark spectra of metals, so that they are no longer to be regarded as unknown lines. Similarly, many of the lines of α Cygni for which no origins could previously be assigned have been shown to be lines of common metals under conditions of high temperature. Still, the mere presence of the enhanced lines in a star spectrum affords us no criterion as to whether the temperature of a star is increasing or decreasing. But I have also shown that if we take the relative intensities of the enhanced lines and the arc lines as an indication of stellar temperatures, and in this way bring together a sufficient number of stars of about the same temperature as γ Cygni or δ Cephei, such spectra may be divided into two well-marked groups, of which γ Cygni and Castor may be taken as types. The chief generic differences between the two groups of stars at the temperature of δ Cephei were thus summarised in the paper to which reference has been made:—

δ Cephei.	Castor.
(1) Considerable absorption in ultra-violet.	(1) Very little continuous absorption in ultra-violet.
(2) Hydrogen lines relatively thin.	(2) Hydrogen lines relatively very thick.
(3) Metallic lines of moderate intensity.	(3) Metallic lines relatively feeble.

I further showed that these differences are simply and sufficiently explained on the supposition that stars like γ Cygni and δ Cephei are uncondensed swarms of meteorites, while those like Castor, which have about the same mean temperature, are stars approaching the condition of the sun in which photospheres and relatively quiescent atmospheres have formed. The foregoing considerations indicate that there are three chief periods in the history of a star during its stages of luminosity:—

(1) A period during which it exists as an uncondensed swarm, when the "atmosphere" is disturbed by meteoritic bombardment from without.

At this stage the atmosphere is a mass of heterogeneous vapour at various temperatures and moving with different velocities in different regions.

(2) A period of complete vaporisation during which the atmosphere is quiescent, bombardment having ceased, and the radiation being too great to permit condensation in the atmosphere.

(3) A period of cooling during which the atmosphere is disturbed by the fall of condensation products from the outer parts of the atmosphere on to the photosphere.

Evidence that we are not dealing with Composite Spectra.

Many of the lines in the spectra of stars of the δ Cephei class coincide with prominent lines in the spectrum of α Cygni, but it is true that many also coincide with lines in the spectra of stars like α Orionis and Arcturus, which closely resemble the solar spectrum. It seemed possible, therefore, that we might be dealing with the integrated spectra of two stars in close proximity, one having lines resembling those of α Orionis or Arcturus, and the other those of α Cygni. The spectra of all the stars of this sub-group have accordingly been very carefully investigated from this point of view. Enlarged glass positives of α Cygni and Arcturus on exactly the same scale have been superposed, and the integrated spectra photographed. When this integrated spectrum is compared with γ Cygni or δ Cephei, there is a considerable similarity, but the relative intensities of the various lines and the general appearance of certain parts of the spectrum, especially about G, are quite different. Again, if there were two bright bodies physically connected in such a star as γ Cygni there must be a revolution and a consequent doubling of the common lines, unless the plane of movement were perpendicular to the line of sight. No signs of such doubling, however, have been detected in any of the eight stars of the sub-group which have so far been recognised, and it is quite improbable that the plane of revolution would be at right angles to the line of sight in every case, and still more so that the two components would have identical spectra in each of the eight systems.

Spectra of the δ Cephei type must, therefore, be taken to represent a particular stage in the orderly development of cosmical bodies.

Evidence from Variability.

Further evidence in favour of placing stars like δ Cephei and γ Cygni on the ascending arm of the temperature curve is afforded by the fact that stars of this class present a special form of variability. This variability is similar in kind, but different in degree, to that associated with stars of Group II, such as Mira. The following table shows that the amount of variation is very much less than that in variables of the Mira type:—

Name.	Period.	Interval from min. to max.	Variation.	Remarks.
	d. h.	d. h.		
δ Cephei.....	5 9	1 15	3·7—4·9	Very regular.
ζ Geminorum	10 4	5 0	3·7—4·5	Period slowly lengthening.
η Aquilæ	7 4	2 9	3·5—4·7	Slightly irregular.
S(10) Sagittæ	8 9	3 10	5·6—6·4	
T Vulpeculæ	4 10	1 7	5·5—6·5	
Mira.....	331 0	..	(1·7—5·0) — (8·7—9·5)	

It will be seen that the luminosity at maximum is from about two (0·8 magnitude) to three times (1·2 magnitudes) greater than at minimum, while the forms of most of the light curves resemble the majority of those of the Mira class in the relatively steep ascent to maximum. A constitution more or less similar to that of the Mira class is therefore indicated.

I have already shown that in such variables as Mira the presence of bright carbon flutings indicates a meteoritic structure.* Here the variation has a much longer period than in δ Cephei, but it is only necessary to suppose that δ Cephei is more condensed, so that revolving swarms of short period will be alone effective in producing collisions at periastron, as I pointed out in 1889.†

A recent discussion of all the available observations of η Aquilæ by Dr. William J. S. Lockyer‡ has shown that the light curve of this variable can be best explained on the supposition of three meteor swarms moving around their centre of gravity. In this way not only is the general form of the light curve satisfied, but the smaller irregularities discovered by the author are also easily accounted for.

Hence, by placing stars of the δ Cephei class on the ascending arm of the temperature curve, the variability of certain members of the group finds a ready explanation. I am not aware of any satisfactory explanation of the δ Cephei type of variability in which a constitution resembling that of the sun is assumed, and to my mind such a variation in a star constituted like the sun is impossible.

* 'Roy. Soc. Proc.' (1887), vol. 43, p. 130.

† 'Roy. Soc. Proc.,' vol. 46, p. 420.

‡ 'Resultate aus den Beobachtungen des veränderlichen Sternes η Aquilæ' (Inaugural-Dissertation, Göttingen, 1897).

General Conclusions.

The final result of the discussion of the spectra of stars of the δ Cephei class is to show that they must be placed on the ascending arm of the temperature curve, at a stage higher than stars like α Tauri, in which the mean temperature is not very different from that of the sun. Stars of equal temperature on the descending side of the curve, of which Castor may be taken as a type, show precisely the same lines, the enhanced and cool lines having the same relative intensities, but with inverted intensities of the hydrogen and metallic lines, and with somewhat less continuous absorption in the ultra-violet. The difference between stars like δ Cephei and those of the sun is therefore partly due to a difference of temperature and partly due to a difference of physical condition such as is demanded by the meteoritic hypothesis. This result enables us to understand why some members of the δ Cephei class should show such a very special kind of variability.

α Cygni also finds a natural place on the ascending arm of the temperature curve, at a stage higher than δ Cephei, and all the difficulties met with in attempting to classify it on Vogel's view of decreasing temperature alone are removed.

"On Lunar and Solar Periodicities of Earthquakes." By Arthur Schuster, F.R.S. Received May 18,—Read June 17, 1897.

1. In a paper recently communicated to the Royal Society "On Lunar Periodicity in Earthquake Frequency," Mr. C. G. Knott gave some results, from which he argued that a real connexion between tidal effects and earthquakes probably existed. These results are based on a method which has frequently been employed. The records of earthquakes are grouped together and expressed by means of a Fourier series, and conclusions are based on the greater or smaller values of the coefficients of this series. In order to decide what value is to be attached to such investigations, it seems necessary in the first instance to discuss what would be the order of magnitude of the coefficients, on the supposition that the events have happened perfectly at random without any connecting law. It is the object of this paper to solve this question, and to apply the solution to the periodicities which are supposed to exist in the frequency of earthquakes.

2. If it is required to investigate a possible period of p intervals of time in a series of members, t_1, t_2, t_3, &c., it is usual to arrange the numbers according to the following scheme, where t_1' stands for t_{p+1}, t_1'' for t_{2p+1}, &c. :—

$$\begin{array}{llll} t_1 & t_2 & t_3 \ldots . & t_p \\ t_1' & t_2' & t_3' \ldots . & t_p' \\ \vdots & \vdots & \vdots & \\ t_1^{s-1} & t_2^{s-1} & \ldots\ldots & t_p^{s-1} \\ \hline \mathrm{T}_1 & \mathrm{T}_2 & \mathrm{T}_3 \ldots . & \mathrm{T}_p \end{array}$$

T_1, T_2, &c., represent the sums of the vertical columns. In the case discussed in the first part of Mr. Knott's paper the intervals are hours, and p is taken to be 25, *i.e.*, approximately equal to the interval between two successive meridian passages of the moon. T_1 would therefore represent the number of earthquakes which have happened within an hour after the meridian passage of the moon.

The numbers T may be expressed by a periodic series of the form

$$\begin{aligned} \mathrm{S} = a_0 &+ a_1 \cos\theta + a_2 \cos 2\theta + \ldots . + a_p \cos p\theta \\ &+ b_1 \sin\theta + b_2 \sin 2\theta + \ldots . + b_{p-1} \sin (p+1)\theta \ldots . \quad (1), \end{aligned}$$

where $\mathrm{S} = \mathrm{T}_1$ if we substitute $\theta = 2\pi/p$, and generally S becomes T_q by the substitution $\theta = 2\pi q/p$.

The coefficients are determined by a well-known process, which gives

$$\left.\begin{aligned} pa_0 &= \mathrm{T}_1 + \mathrm{T}_2 + \ldots\ldots\ldots\ldots + \mathrm{T}_p \\ \tfrac{1}{2}pa_1 &= \mathrm{T}_1 \cos\theta + \mathrm{T}_2 \cos 2\theta + \ldots . + \mathrm{T}_p \cos p\theta \\ \tfrac{1}{2}pb_1 &= \mathrm{T}_1 \sin\theta + \mathrm{T}_2 \sin 2\theta + \ldots . + \mathrm{T}_p \sin p\theta \end{aligned}\right\} \theta = 2\pi/p \ldots . \quad (2).$$

The amplitude of the first periodic term would be $r_1 = \sqrt{a_1^2 + b_1^2}$.

It is seen that a_0 is equal to the mean value of all the quantities T or to s times the mean values of all the quantities t.

From the above equations it follows that—

$$\frac{r_1^2}{4a_0^2} = \frac{(\mathrm{T}_1 \cos\theta + \mathrm{T}_2 \cos 2\theta + \ldots + \mathrm{T}_p \cos p\theta)^2 + (\mathrm{T}_1 \sin\theta + \mathrm{T}_2 \sin 2\theta + \ldots + \mathrm{T}_p \sin p\theta)^2}{(\mathrm{T}_1 + \mathrm{T}_2 + \ldots \mathrm{T}_p)^2}.$$

$$\ldots\ldots \quad (3).$$

We may take the quantity $\rho = r_1/a_0$ as a measure of the periodicity corresponding to p intervals. Our problem now is this: "*What is the probability that ρ should lie between any two assigned values ρ_1 and ρ_2, on the supposition that the events are all distributed at random.*" The problem may be put into a more general form. The events, like the earthquakes in Mr. Knott's paper, have all been put into the same compartment if they happened within certain interval of time (an hour in this case), no matter whether they happened at the beginning or at the end of that interval. This simplification is introduced only for purposes of more easy arithmetical calculation. Theoretically,

and if we had a sufficient quantity of material, it would be better to reduce the lengths of the intervals and to increase their number p. If this process is carried sufficiently far equations (2) become

$$pa_0 = n,$$

$$\tfrac{1}{2}pa_1 = \cos \kappa t_1 + \cos \kappa t_2 + \ldots + \cos \kappa t_n,$$

$$\tfrac{1}{2}pb_1 = \sin \kappa t_1 + \sin \kappa t_2 + \ldots + \sin \kappa t_n \quad \ldots\ldots\ldots \quad (4),$$

where n is the total number of events and κ stands for $2\pi/T$, T being the whole length of the period, and t_1, t_2, &c., the times of occurrence of successive events.

Equation (3) will become

$$\frac{n}{2}\frac{r_1}{a_0} = \{(\cos \kappa t_1 + \cos \kappa t_2 + \ldots \cos \kappa t_n)^2 + (\sin \kappa t_1 + \sin \kappa t_2 + \ldots + \sin \kappa t_n)^2\}^{\frac{1}{2}} \quad \ldots\ldots\ldots \quad (5).$$

The meaning of the expression on the right-hand side is best illustrated by means of a diagram. On a circle with centre at O and unit radius, take points P_1, P_2 such that the angles between the lines OP_1, OP_2 and a fixed direction are kt_1, kt_2, &c. If OP_1, OP_2 represent forces of equal intensity but different directions, the right-hand side of (5) gives the magnitude of the resultant force. As, according to hypothesis, the events may happen with equal probability at any time, every position on the circle is equally probable for every point P. Under these circumstances it has been shown by Lord Rayleigh* in a paper "On the Resultant of a large number of Vibrations of the Same Pitch and Arbitrary Phase," that the probability of the resultant having a value lying between s and $s+ds$ is

$$\frac{2}{n}e^{-s^2/n}\,sds \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (6),$$

n being the total number of vectors combined. It is a simple matter to pass from this result to the solution of our problem.

From (5) and (6) it follows that the probability for the value of $nr_1/2a_0$ lying between $\frac{1}{2}n\rho$ and $\frac{1}{2}n(\rho+d\rho)$ is

$$\frac{n}{2}\rho e^{-\frac{1}{4}n\rho^2}\,d\rho \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (7),$$

and this is therefore also the probability that r_1/a_0 has a value intermediate between ρ and $\rho+d\rho$.

The expectancy for r_1/a_0 is

$$\frac{n}{2}\int_0^\infty \rho^2 e^{-\frac{1}{4}n\rho^2}\,d\rho = \sqrt{\frac{\pi}{n}} = \frac{1{\cdot}77}{\sqrt{n}} \quad \ldots\ldots\ldots\ldots \quad (8).$$

* 'Phil. Mag.,' vol. 10, p. 73 (1880, II).

The probability that the value of r_1/a_0 exceeds ρ is

$$\frac{n}{2}\int_{\rho}^{\infty} \rho\, e^{-\frac{1}{4}n\rho^2} d\rho = e^{-\frac{1}{4}n\rho^2} \quad \ldots\ldots\ldots\ldots\ldots \quad (9).$$

If the higher coefficients $r_2 = \sqrt{a_2^2+b_2^2}$, &c., are treated in the same manner, the same expression is found to hold.

Our final result may now be expressed as follows:—

If a number n of disconnected events occur within an interval of time T, all times being equally probable for each event, and if the frequency of occurrence of these events is expressed in a series of the form

$$a\left(1+\rho_1 \cos 2\pi \frac{t-t_1}{T}+\rho_2 \cos 4\pi \frac{t-t_2}{T}+ \ldots\ldots +\rho_n \cos 2p\pi \frac{t-t_n}{T}\right),$$

the probability that any of the quantities ρ has a value lying between ρ and $\rho+d\rho$ is

$$\frac{n}{2}\rho e^{-n\rho^2/4} d\rho,$$

and the expectancy for ρ is

$$\sqrt{\pi/n}.$$

In proving this proposition it was assumed that the number of intervals into which the period T is subdivided is very large, but this condition is not essential. To suit accurately the process employed by Mr. Knott, we should have to consider the vectors OP_1, OP_2, &c., to be confined to fixed directions forming angles $2\pi/p$ with each other. But it follows directly from the method employed by Lord Rayleigh that his results must apply to this case also, if p is a multiple of four. It is further not necessary to enquire whether (7) holds in the most general case, when p, for instance, is an odd number, because the process employed by Mr. Knott and others is justified only on the assumption that the number of intervals into which the period is subdivided is so large that a further increase of it would not alter the relative value of the coefficients of the Fourier series.

3. Unfortunately Mr. Knott has adopted a common but in my opinion mischievous practice, which renders some further reductions necessary before we can apply expressions (7) and (8). Instead of basing his calculations on the number of earthquakes which took place in any particular lunar hour, he first takes overlapping means of the numbers put down for five consecutive hours. This practice has its legitimate use, when it is desired to make periodicities apparent to the eye by plotting down a series of numbers which by themselves may be too irregular to bring out the peculiarities which it is intended to show. The averaging of a

certain number of successive figures eliminates to some extent the shorter periods, and therefore emphasises the longer ones. But this elimination is done in a much better and more complete manner by Fourier's analysis, and when it is therefore intended to submit the figures to that process of calculation, the smoothing down of the original numbers, represents a waste of arithmetical work, uncompensated, as far as I can see, by any advantage.

To judge of Mr. Knott's results it is necessary therefore to trace the effect of the process of calculation employed by him. The problem to be solved for that purpose may be stated thus :—

A number, p, of figures, $y_1, y_2 \ldots y_p$, is expressed in terms of the periodic series (1). A second series is then formed by taking the sum of m successive numbers, the first two members of the series being $y_1+y_2+\ldots y_m$ and $y_2+y_3+\ldots y_{m+1}$. The original series is supposed to repeat itself, so that the last member of the derived series becomes $y_p+y_1+y_2+\ldots +y_{m-1}$. It is required to find the relation between the coefficients of the Fourier expansion for the two series.

It will be sufficient to find the solution of this problem for the case that the original series y_1, y_2 represent equidistant ordinates of the curve $y = \cos \kappa x$, so that

$$y_1 = \cos \kappa x_1, \quad y_2 = \cos (\kappa x_1 + \delta), \quad y_3 = \cos (\kappa x_1 + 2\delta) \ldots .$$

$$y_m = \cos (\kappa x_1 + (m-1)\delta).$$

The solution of this case really includes the general one, for the original series y is supposed to be represented by a number of terms, each of which is simply periodic, and to each of which the result of the special case may be separately applied.

The first term of the derived series may be obtained by a well known process, for

$$y' = y_1+y_2+ \ldots . y_m = \frac{\sin \frac{1}{2} m\delta}{\sin \frac{1}{2}\delta} \cos \left(\kappa x_1 + \frac{m-1}{2} \delta \right).$$

The subsequent terms are obtained simply by altering the value of x_1.

The derived series, y', y'', &c., represents therefore the equidistant ordinates of the curve

$$y = \frac{\sin \frac{1}{2} m\delta}{\sin \frac{1}{2}\delta} \cos \left(\kappa x + \frac{(m+1)\delta}{2} \right).$$

It is a curve having the same period as the original one, but having an amplitude reduced in the ratio $\sin \frac{1}{2} m\delta / \sin \frac{1}{2}\delta$. Applying this result to the separate terms of the series (1) and remembering that δ represents the difference in phase between two successive

terms of the original series, it is seen that the coefficients a_q and b_q are both reduced in the ratio $\sin\frac{\pi mq}{p} \Big/ \sin\frac{\pi q}{p}$. This result has to be divided by m to fit it to the case that the derived series is not formed by taking the *sum* of m successive numbers, but by taking their *mean* value.

The effect of smoothing down the irregularities of the observed numbers has therefore the effect of reducing the qth coefficients of the Fourier series in the ratio

$$\sin\frac{\pi mq}{p} \Big/ m \sin\frac{\pi q}{p}.$$

In the case under discussion, $m = 5, p = 25$, so that the amplitudes of the first four coefficients of the series are reduced respectively in the ratios

0·938, 0·765, 0·517, 0·244.

The number of earthquakes taken into account was 7427, so that the expectancy for the amplitude of any one of the coefficients, irrespective of the smoothing process, would be

$$\sqrt{\pi/7427} = 0{\cdot}0206.$$

If this number is multiplied by the above fractions, and then by 1000 in order to make the units agree with those of Mr. Knott's paper, we obtain the numbers placed in the first row of Table I; the second row gives by comparison the coefficients actually found by Mr. Knott.

Table I.

Coefficients.	C_1.	C_2.	C_3.	C_4.
Expectancy for the coefficients by the theory of probability	19·3	15·7	10·6	5·02
Coefficients found by Mr. Knott.....	10·3	17·9	10·9	3·97

The numbers in this table do not support Mr. Knott's contention, but seem to me rather to be a striking confirmation of the theory of probability. It must be remembered that the "expectancy" only gives the average value of a great many cases, the individuals of which may differ considerably from that average. Thus it may be calculated with the help of expression (9) that in about one case out of every four the coefficient C_1 would come out still smaller than the number found by Mr. Knott, while the coefficient C_2 would be larger

than Mr. Knott's number in two out of every five cases. It will, I think, be admitted that, until further evidence is brought forward, the lunar day cannot be considered to affect earthquakes.

4. In the second part of his paper, Mr. Knott treats of the monthly and fortnightly periodicities. He expresses himself very guardedly as to the results of this part of the investigation, chiefly on the ground that the amplitudes found for the period of the "nodical" month are quite as great as those found for the "tropical" or "synodic" months. As there is no conceivable reason why the nodical month should affect earthquakes, the conclusion will reasonably be drawn from this, that any period chosen at random would give similar results. I think we must take it, in default of further evidence, that this argument is valid and outweighs the not very conclusive considerations which seem to Mr. Knott to favour the reality of these lunar periods, in spite of the weakness of the evidence to which he himself draws attention. The number of earthquakes taken into consideration are about 4730, and this would give an expectancy for each of the coefficients equal to 0·0258. A reducing factor has to be applied which differs for the different coefficients as has been explained in the case of the periods depending on lunar day. I obtain in this way the numbers shown in Table II. For the sake of completeness, I give the amplitudes found by Mr. Knott for the various months, and also the mean amplitudes of the different monthly periods.

Table II.

"Month."	C_1.	C_2.	C_3.	C_4.
(1) Anomalistic	46·2	47·8	12·9	16·5
(2) Tropical	54·7	40·7	23·1	17·2
(3) Nodical	49·5	55·2	28·3	17·6
(4) Synodic	11·0	52·1	24·5	4·7
Mean of (1), (2), (3), (4)	40·4	49·0	22·2	14·0
Expectancy by theory of probability ...	24·5	20·9	15·5	9·3

The striking feature of this table consists in the first place in the roughly equal amplitudes for the different kinds of months, and secondly in the fact that the expectancy is in all cases smaller than the mean of the coefficients found, and in nearly equal proportions at any rate for the monthly, third-monthly, and quarter-monthly periods.

5. This points not so much to the reality of the period found, but to some common cause which has led to too low an evaluation of the expectancy. The latter quantity has been calculated under the

assumption that the earthquakes are altogether independent of each other and take place at random. Any regularity would be contrary to this assumption and might affect the expectancy. Thus, for instance, it is known that earthquakes take place in groups, a large earthquake being generally followed by some minor shocks.

Mr. Knott states that the "obvious" aftershocks were left out of account in his calculation; but there may be aftershocks which are not obvious, and it seems quite likely that every earthquake is followed by a period during which another is more likely to happen than at other times. If there is such a tendency it is easy to see that our calculated numbers for the expectancy will be too low. Take the case, for instance, that all earthquakes happen in groups of two, or, what comes to the same thing, let each earthquake in the investigation of § 2 count as two. The quantities I have called ρ will not be affected by this change, but the total number of earthquakes being doubled, the expectancy calculated according to our formula is now reduced in the proportion of $\sqrt{2}$ to 1, and would therefore be too small in that ratio. It seems to me to be probable that the discrepancy between Mr. Knott's coefficients and the calculated expectancy is explained in this way. Apart from this possible explanation it would not be safe to draw any certain conclusions from any instance in which the calculated amplitude has double the value of the expectancy, for expression (9) shows that the amplitudes will turn out to be even greater than that on the average in one case out of every twenty-three. The matter to be explained is not that any one of Mr. Knott's coefficients is, roughly speaking, twice as great as the expectancy, but that all coefficients show this tendency towards higher values in not very different proportions.

6. It is interesting to discuss, from the point of view of this paper, the periodicities of earthquakes which apparently depend, directly or indirectly, on the position of the sun. We owe to Mr. Davison a very complete discussion of the annual period.* The method employed by him (in determining the amplitudes of the periodic terms), though neither direct nor very accurate, is sufficient for our purpose. Taking as an example the record of 5879 earthquakes in the northern hemisphere, given by Mr. R. Mallet, the results of this paper show that if they were distributed indiscriminately over the whole year the expectancy for the amplitude would be $\sqrt{\pi/5879}$, or 0·023, while Mr. Davison, in § 18 of the paper quoted, gives 0·11 for the amplitude. Similarly, the discussion of 8133 earthquakes, for which the expectancy is 0·020, yields the number 0·29 for the amplitude. Here, then, we have the amplitude in one case equal to five times, and in the other equal to fourteen times, the expectancy.

The probability of the accidental nature of so large an amplitude

* 'Phil. Trans.,' A, vol. 184, p. 1107 (1893).

is in the first case only 1 in 300,000, and in the second almost infinitesimally small. The reality of the period would be thereby established beyond reasonable doubt, unless the peculiarity of earthquakes occurring in groups, as discussed in the previous section, can be shown to raise the expectancy sufficiently. The fact, however, that in each hemisphere the phase of the periodicity found is nearly identical in a great number of cases disposes of all doubt which might remain on that point.

In some of the details Mr. Davison's results would seem to require further confirmation. The evidence, for instance, that the strong and weak shocks follow different laws is not very strong when examined by means of the theory of probability. Mr. Davison classifies the Japanese earthquakes into three groups, according to their intensity, and finds that the maxima of the annual period agree in the two stronger groups and take place in winter, while the maxima for the weakest group occur in summer. The magnitude of the amplitude for the former is 0·17, while the expectancy is 0·074 and 0·133 respectively. Here the excess of amplitude over the expectancy is not sufficiently marked to allow of any certain conclusions being drawn. A similar remark applies to the record of Zante (see § 45 of Mr. Davison's paper).

7. The daily periods of earthquakes have been fully discussed by the same author in a paper published in the 'Philosophical Magazine.' The following table summarises some of the more important results, the twenty-four hours period only being taken into account. The epoch given is that of the maximum, and I have added the expectancy of amplitude calculated on the principles of this paper.

	Number of earthquakes, n.	Expectancy, $\sqrt{\frac{\pi}{n}}$.	Amplitude.	Epoch.
Japan (Tokio), Summer .	543	0·076	0·176	9h 58m A.M.
Winter..	661	0·069	0·093	10 39
Year....	1204	0·051	0·130	10 14
Japan, Summer.....	597	0·073	0·061	0 2 P.M.
Winter	578	0·074	0·239	11 50 A.M.
Year	1175	0·052	0·147	11 53
Philippine Islands	210	0·122	0·273	10 49
Italy	8177	0·020	0·324	0 25 P.M.

The amplitudes are seen to exceed the expectancy considerably in all cases but one. The reality of the daily period must be considered established, unless the evaluation of the expectancy is faulty, owing

to the fact that tremors occur in groups (§ 5). The good agreement between the phases of the periodicity disposes of that doubt.

8. The expression for the expectancy deduced in § 2 may with advantage be employed in similar investigations to decide the number of events which it is necessary to take into account in order to establish a periodicity of given amplitude. It follows from the laws of probability that, to be reasonably certain, the amplitudes found should be at least equal to three times the expectancy. Hence, if c be the amplitude looked for,

$$c = 3\sqrt{\pi/n}, \quad \text{or} \quad n = 9\pi/c^2 = 28{\cdot}3/c^2.$$

Thus, for instance, Mr. Knott deduces for his supposed lunar period a range of 6 per cent., or an amplitude of 0·03. In order to establish with certainty such an amplitude, it would be necessary that the number of earthquakes taken into account should at least be equal to 30,000, or over four times the number actually used in the calculations.

9. The difficulty discussed in § 5 would seem to limit in many cases the applicability of the results found. There are, indeed, in nearly every case actually occurring in nature, certain regularities in the manner in which events happen, and this regularity always favours the higher values of the Fourier coefficients. As it will not be possible to estimate in many cases the effect of the expectancy, some other form of treatment will often be called for. The following theorem will, I think, prove useful in these investigations :—

Let y be a function of t, such that its values are regulated by some law of probability, not necessarily the exponential one, but acting in such a manner that if a large number of values of t be chosen at random there will always be a definite fraction of that number depending on t_1 only, which lie between t_1 and $t_1+\mathrm{T}$, where T is any given time interval.

Writing $\quad \mathrm{A} = \int_{t_1}^{t_1+\mathrm{T}} y \cos \kappa t dt \quad$ and $\quad \mathrm{B} = \int_{t_1}^{t_1+\mathrm{T}} y \sin \kappa t dt,$

and forming

$$\mathrm{R} = \sqrt{\mathrm{A}^2+\mathrm{B}^2},$$

the quantity R will, with increasing values of T, fluctuate about some mean value, which increases proportionally to $\sqrt{\mathrm{T}}$, provided T is taken sufficiently large.

If this theorem is taken in conjunction with the two following well-known propositions,

(1) If $y = \cos \kappa t$, R will, apart from periodical terms, increase proportionally to T;

(2) If $y = \cos \lambda t$, λ being different from κ, the quantity R will fluctuate about a constant value;

it is seen that we have means at our disposal to separate any true periodicity of a variable from among its irregular changes, provided we can extend the time limits sufficiently.

The proof of this proposition lies outside the limits of this paper.

The application of the theory of probability to the investigation of what may be called "hidden" periodicities, an instance of which has here been given, may be further extended, and interesting results are obtained when a number of periodicities, such as those supposed to depend on the rotation of the sun about its axis, are critically examined. A full treatment of the subject will shortly appear in 'Terrestrial Magnetism.'

"The Vector Properties of Alternating Currents and other Periodic Quantities." By W. E. SUMPNER, D.Sc. Communicated by O. HENRICI, F.R.S. Received May 28,—Read June 17, 1897.

It has been well known for many years that the variations of a simple harmonic function, such as $A \cos pt$, can be represented by the projection, on a fixed line, of a vector of constant length A supposed to revolve uniformly so as to complete one revolution in a time T given by the relation $pT = 2\pi$. The angle between the revolving vector and the fixed line is pt at any instant t. In the first edition of Thomson and Tait's 'Natural Philosophy' (vol. 1, p. 38, § 58), it is shown that any two simple harmonic functions of one period can be compounded to a single simple harmonic function of the same period, and that the vector, representing the compounded function, is obtained from those representing the component functions by the ordinary process of vector addition.

This device has proved useful for many purposes, but it has proved especially valuable in connexion with alternate current problems. Its application to such cases was first clearly pointed out by Mr. T. H. Blakesley more than twelve years since. The use of it in alternate current work has gradually developed into what may be described as a vector, or network, method of representing alternate current quantities. In this method the length of the vector denotes the magnitude of the current or voltage, while the angle between any two vectors represents the time which elapses between the instants at which the maximum values of the corresponding quantities occur. In particular, if one line represents the voltage on a conductor, and a second line denotes the strength of current produced in it by this voltage, the power absorbed in the conductor is measured by the product of the length of the two lines into the cosine of the angle between them. It is to this possibility of

deducing from the network figure the electrical *power* absorbed by the conductors, as well as the magnitude of each current, and voltage, that is chiefly due the usefulness and importance of this mode of representation.

It will be convenient here to describe briefly the mathematical basis on which the method rests. It will be evident that we may represent the value of a simple harmonic function $A \cos pt$ either by the projection of a revolving vector on a fixed line, or by the projection of a stationary vector on a revolving line. It will be more convenient for our purpose to adopt the latter plan. If, therefore, we have a vector α, of length A, the projection of this on a line supposed to revolve uniformly will always be equal to $A \cos pt$, provided the period of revolution T is such that $pT = 2\pi$, and provided also that the line coincides with the vector at the instant from which t is measured. We can, moreover, represent the value of $B \sin pt$ by the projection at the *same instant* on the *same revolving line* of a vector β of length B, provided only that the vectors α and β are perpendicular. It will also be evident that any other simple harmonic function of the same period

$$mA \cos pt + nB \sin pt$$

will be similarly represented by the projection of the vector

$$m\alpha + n\beta,$$

and the length of this vector will be equal to the maximum value of the function it represents.

Now, in any system of conductors traversed by alternate currents, if every electromotive force E_x is a simple harmonic function of one period, $E_x = V_x \sin(pt + \phi_x)$, and if the coefficients of induction and the capacities are all constant quantities, it follows mathematically that each current, and each voltage, can be expressed in the general form

$$F = \lambda \cos pt + \mu \sin pt,$$

which can thus be represented by the vector

$$v = \lambda\alpha + \mu\beta,$$

where α and β are the perpendicular vectors representing $\cos pt$ and $\sin pt$.

If α and β are unit vectors, the length of v is $\sqrt{(\lambda^2 + \mu^2)}$, which is the maximum value of the function F. The *magnitude* of an alternate current quantity is however estimated by the square root of its mean square, and not by its maximum value, and since

$$\frac{1}{T}\int_0^T F^2 dt = \tfrac{1}{2}(\lambda^2+\mu^2),$$

if we choose for α and β two perpendicular vectors of lengths numerically equal to $1/\sqrt{2}$, the length of $v = \lambda\alpha+\mu\beta$ will be equal to $\sqrt{\tfrac{1}{2}(\lambda^2+\mu^2)}$, or to the *magnitude* of F.

If now we consider the value of the mean product of two functions $F_1 \times F_2 =$

$$(\lambda_1 \cos pt+\mu_1 \sin pt)\,(\lambda_2 \cos pt+\mu_2 \sin pt),$$

we see that this mean product is

$$\tfrac{1}{2}(\lambda_1\lambda_2+\mu_1\mu_2),$$

or the same as the scalar product of the corresponding vectors,

$$(\lambda_1\alpha+\mu_1\beta)\,(\lambda_2\alpha+\mu_2\beta).$$

Every alternating current problem is reducible to the consideration of the current, voltage, and electrical power spent in the different conductors. Since all these quantities are varying periodically some average value has to be taken. For current and voltage the mode of averaging selected is always that obtained by taking the square root of the mean value of the square of the quantity during the period. The electrical power is, however, the mean value through the period of the product of the instantaneous values of voltage and current. Hence, if the current and voltage are represented by vectors in the manner described, the scalar product of the two vectors will represent the power. This vector method is particularly suited to alternate current problems, and has been very much used for such purposes by electricians, since it is simple to understand and to apply, and it can be used for the simple solution of many problems which would otherwise need the use of the calculus.

The method, however, assumes that all the voltages and currents considered vary according to a simple sine law. In ordinary commercial practice this is unfortunately not the case, owing chiefly to saturation and hysteresis phenomena in the magnetic circuits, and also to the fact that ordinary dynamos do not develop an electromotive force varying according to a simple harmonic law. The object of the present investigation is to show how far this method can be modified, and used correctly, in cases where the currents, although periodic, do not vary according to any simple law.

Since we shall have frequently to refer to the value of the square root of the mean square of a periodic function, we shall call such a

quantity the *magnitude* of the periodic function. Thus, if v is a varying function of the time, periodically repeating itself after every interval T, the *magnitude* of v, which we shall denote by $\overset{=}{v}$, is given by the equation

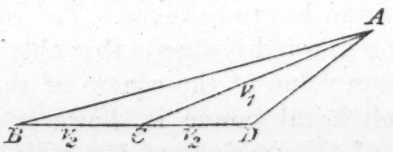
$$\overset{=}{v} = \textit{magnitude of } v = \sqrt{\frac{1}{T}\int_0^T v^2 dt}.$$

When we are considering two such functions, v_1 and v_2, the mean value of their product will be denoted by

$$\overline{v_1 v_2} = \frac{1}{T}\int_0^T v_1 v_2 dt.$$

The mode of measuring electrical power known as the Three Voltmeter Method,* is really dependent upon a curious property of periodic functions which may be stated as follows:—

If *any two arbitrary periodic functions*, v_1 and v_2, be represented in magnitude by the lengths of two sides of a triangle, and if the third side represents the magnitude of v (another periodic function equal



to the sum, or difference, of the former two), then the mean value of the product of any two of these functions will be represented by the product of the lengths of the two corresponding sides and the cosine of the angle between them.

Thus, in the figure, if CA represents v_1, and BC (or CD) represents v_2, then BA will represent v_1+v_2, and DA will represent v_1-v_2; also the mean product of $(v_1+v_2)v_2$ will be $BA \times BC \cos ABC$, the mean product $(v_1-v_2)v_1$ will be $DA \times CA \cos CAD$, and so on. Thus we may regard v_1 and v_2 as vectors, the length of the vector representing the magnitude of the periodic function, and the scalar product of two vectors the mean product of the two corresponding functions. Any periodic function derivable from v_1 and v_2, by a linear relation, can be similarly represented in accordance with the ordinary properties of vectors, and the scalar product of any two such vectors will be equal to the mean product of the corresponding periodic functions.

In order to establish this we may proceed as follows:—

* See "The Measurement of the Power given by any Electric Current to any Circuit," Professor W. E. Ayrton and Dr. W. E. Sumpner, 'Roy. Soc. Proc.,' April 9, 1891.

Whatever be the law of variation of v, v_1, and v_2, if

$$v = v_1 \pm v_2 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1),$$

then $$v^2 = v_1^2 \pm 2v_1v_2 + v_2^2$$

and, taking means, we have

$$\overline{v^2} = \overline{v_1^2} \pm 2\overline{v_1v_2} + \overline{v_2^2},$$

which equation is the same as

$$(\bar{v})^2 = (\bar{v}_1)^2 \pm 2\overline{v_1v_2} + (\bar{v}_2)^2 \ldots\ldots\ldots\ldots\ldots (2).$$

But if the lengths of the three sides of a triangle respectively represent $\bar{v}$, $\bar{v}_1$, and $\bar{v}_2$, we have from geometry

$$(\bar{v})^2 = (\bar{v}_1)^2 + (\bar{v}_2)^2 \pm 2(\bar{v}_1)(\bar{v}_2)\cos\phi \ldots\ldots\ldots\ldots (3),$$

where ϕ is the angle between $\bar{v}_1$ and $\bar{v}_2$.

Comparing equations (2) and (3) with the figure, it will be seen that for the case represented by the line AB the positive sign must be taken in each equation, while for the other case, AD, the negative signs must be chosen. In either case we have $\overline{v_1v_2} = (\bar{v}_1)(\bar{v}_2)\cos\phi$. Moreover, if in equation (1) we transpose either v_1 or v_2, and square and proceed as before, we find that whichever two sides of the triangle ABC be chosen, the products of these sides into the cosine of the angle between them is equal to the mean product of the corresponding periodic functions.

The foregoing argument shows that *assuming it possible* to form a triangle with sides equal to the *magnitudes* of v, v_1, and v_2, such triangle has the properties stated. But we have yet to show that it is always possible to construct such a triangle, or, in other words, to show that any two of these quantities are together necessarily greater than the third.

Now if $$v = v_1 + v_2,$$

we find by squaring and taking the mean that

$$(\bar{v})^2 = (\bar{v}_1)^2 + (\bar{v}_2)^2 + 2\overline{v_1v_2};$$

we shall therefore prove that

$$\bar{v} < \bar{v}_1 + \bar{v}_2,$$

provided we can establish the inequality

$$(\bar{v}_1)(\bar{v}_2) > \overline{v_1v_2} \ldots\ldots\ldots\ldots\ldots\ldots (4);$$

so that if we can show for *any* two periodic functions that the product of their magnitudes is necessarily greater than the mean value of their product, we shall have shown that it is always possible to construct a triangle the sides of which represent the magnitudes of any three periodic functions related as in equation (1).

The meaning of (4) can be illustrated by supposing that v_1 represents an alternating voltage applied to a conductor, and producing a current, v_2, through it. The right-hand element of (4) then represents the power absorbed, while the left-hand element is the product of volts and amperes. The fact that the power absorbed in an inductive circuit is always found to be less than the product of volts and amperes, is usually stated by asserting that the "power factor" is less than unity for such circuits. Experience has not shown any case in which the power factor of a circuit is greater than unity, but this of course is no evidence of the truth of (4), which must be proved for the general case in which the two periodic functions are perfectly arbitrary Fourier series.*

Let us therefore put

$$v_1 = \sum_m (A_{1m} \sin mpt + B_{1m} \cos mpt) \quad \ldots\ldots\ldots \quad (5).$$

$$v_2 = \sum_m (A_{2m} \sin mpt + B_{2m} \cos mpt) \quad \ldots\ldots\ldots \quad (6).$$

By taking the mean value of twice the square of each of these equations, we get

$$2\overline{(v_1)}^2 = \sum_m (A_{1m}^2 + B_{1m}^2),$$

$$2\overline{(v_2)}^2 = \sum_m (A_{2m}^2 + B_{2m}^2),$$

and by taking four times the value of the mean product we get

$$4\overline{v_1 v_2} = 2 \sum_m (A_{1m}A_{2m} + B_{1m}B_{2m}),$$

* In the proof given, the two functions v_1 and v_2 are assumed to have fundamental periods that are commensurable with each other. This will always be so in practical cases, and what happens with functions of incommensurable periods is of but theoretical interest. It is, however, worth notice that no limitation is thereby imposed on the application of the theorems given in this paper. These theorems deal with mean products, and the truth of them is directly deducible from the fundamental inequality given in (4). Now when the periods are incommensurable the product $v_1 v_2$ consists of terms such as $\sin mpt \sin npt$, where m and n are incommensurable, and as each of these can be expressed as the sum of two sinuous functions of angles denoted by $(m \pm n)\,pt$, the mean value is zero, the vectors representing v_1 and v_2 are perpendicular, and the truth of (4) is at once evident. But inasmuch as the value of $(m-n)$ may be infinitesimally small, it may be necessary to take an infinite time to determine the mean values.

and we shall have proved (4) if we can show that

$$4\left(\overline{v_1}\right)^2\left(\overline{v_2}\right) > \left(2\overline{v_1v_2}\right)^2,$$

or that

$$\{\Sigma_m (A_{1m}^2+B_{1m}^2)\}\times\{\Sigma_m (A_{2m}^2+B_{2m}^2)\}>\{\Sigma_m (A_{1m}A_{2m}+B_{1m}B_{2m})\}^2.$$

This will be proved if we can establish the algebraic inequality—

$$(A_1^2+A_2^2+\ldots.+A_n^2)\,(B_1^2+B_2^2+\ldots.+B_n^2) > (A_1B_1+A_2B_2+\ldots.+A_nB_n)^2 \ldots. (7).$$

Now it will be found quite easy to establish this inequality by an inductive method, but there is no need to do so here, since it is proved in the theory of determinants that

$$\begin{vmatrix} A_1^2+A_2^2+\ldots.+A_n^2 & A_1B_1+A_2B_2+\ldots.+A_nB_n \\ A_1B_1+A_2B_2+\ldots.+A_nB_n & B_1^2+B_2^2+\ldots.+B_n^2 \end{vmatrix} = \Sigma \begin{vmatrix} A_1 & A_2 \\ B_1 & B_2 \end{vmatrix}^2,$$

and is necessarily positive.

It follows that the inequality (7) is true. In order that the inequality may become an equality the necessary and sufficient conditions are

$$A_1/B_1 = A_2/B_2 = \ldots. = A_n/B_n.$$

In other words, the two Fourier series must bear a constant ratio to each other at every instant of time, and must be multiples of the *same* Fourier function. This is a limiting case which is not contemplated in (4), and with this exception only, we have shown* that :—

* [The following much simpler proof has recently been noticed :—
Whatever v_1 and v_2 may be, we can always put

$$v_1 = a_1+x, \qquad v_2 = a_2+y,$$

where a_1 and a_2 are scalar constants given by the equations

$$a_1 = \overline{v_1}, \qquad a_2 = \overline{v_2}.$$

By squaring each expression and taking means we get

$$0 = 2a_1\bar{x}+\overline{x^2}, \qquad 0 = 2a_2\bar{y}+\overline{x^2}+\overline{y^2},$$

where by $\bar{x}$ is meant the arithmetical mean of x, &c.
Also, we obtain by multiplication and taking means,

$$\overline{v_1v_2} = a_1a_2+\overline{xy}+a_1\bar{y}+a_2\bar{x}.$$

The product of the magnitudes of two periodic functions is always greater than the mean value of their product.

From this and from the foregoing reasoning, it follows that :—

Any two periodic functions can be represented by vectors in such a way that the length of each vector represents the magnitude of the function, and the scalar product of the vectors, the mean product of the functions. Any other function derived from the first two by means of a linear relation can be represented in magnitude by a vector derived by means of the same linear relation from the two original vectors. The scalar product of any two such vectors will be equal to the mean product of the corresponding functions.

Now suppose α and β are two vectors representing two arbitrary Fourier series, F_α and F_β. It follows from the law of vectors that any vector in the plane $\alpha\beta$ will be a linear function of α and β, and must therefore represent a Fourier series derivable from F_α and F_β in a linear manner. If, therefore, we have a third arbitrary Fourier series, F_γ, which cannot be expressed as a linear function of F_α and F_β, it follows that this cannot be represented by a vector in the same plane as α and β. It remains to show that a vector, γ, out of this plane can still be found to represent F_γ.

Let us first of all assume that three vectors α, β, γ (not in one plane) have been found to represent three Fourier series F_α, F_β, and F_γ. We can then show that any linear function of α, β, γ, similarly represents the Fourier series, which is the same linear function of F_α, F_β, F_γ.

For if, in accordance with the notation already used, we denote by the symbols

$\overline{\overline{F_\alpha}}$	the square root of the mean square of the value of F_α, *i.e.*, the *magnitude* of F_α,
$\overline{F_\beta F_\gamma}$	the mean value of the product $F_\beta F_\gamma$,
$\overline{\overline{\alpha}}$	the length of the vector α,
$\overline{\beta\gamma}$	the scalar product of the vectors β, γ,

If we substitute from the above relations, we get

$$\overline{v_1 v_2} = a_1 a_2 + \overline{xy} - \tfrac{1}{2}\left\{\frac{a_1}{a_2}\overline{y^2} + \frac{a_2}{a_1}\overline{x^2}\right\} = a_1 a_2 - \frac{\overline{Z^2}}{2a_1 a_2},$$

where Z represents the value of $a_2 x - a_1 y$.

It follows that $\overline{v_1 v_2}$ is always less than $a_1 a_2$, except in the limiting case, in which Z is zero at all parts of the period. This will only occur if

$$a_2 x = a_1 y$$

at every instant, or when

$$a_2 v_1 = a_1 v_2.$$

—June 11, 1897.]

we then have $\overline{\overline{\alpha}} = \overline{\overline{F_\alpha}}$, $\overline{\overline{\beta}} = \overline{\overline{F_\beta}}$, $\overline{\overline{\gamma}} = \overline{\overline{F_\gamma}}$,

$$\overline{\beta\gamma} = \overline{F_\beta F_\gamma}, \quad \overline{\gamma\alpha} = \overline{F_\gamma F_\alpha}, \quad \overline{\alpha\beta} = \overline{F_\alpha F_\beta}.$$

If, now, we compare the value of the square of any vector $\lambda\alpha+\mu\beta+\nu\gamma$ with the value of the mean square of the corresponding Fourier series $\lambda F_\alpha+\mu F_\beta+\nu F_\gamma$, we find, by expanding and using the above relations, that the expressions are identical; and, moreover, it is clear that the scalar product of any two such vectors

$$(\lambda_1\alpha+\mu_1\beta+\nu_1\gamma)\,(\lambda_2\alpha+\mu_2\beta+\nu_2\gamma)$$

is identical with the mean value of the product of the corresponding pair of Fourier series

$$(\lambda_1 F_\alpha+\mu_1 F_\beta+\nu_1 F_\gamma)\,(\lambda_2 F_\alpha+\mu_2 F_\beta+\nu_2 F_\gamma).$$

We have now, however, to show that whatever F_α, F_β, F_γ may be, it is always possible to find three vectors, α, β, γ, to represent them.

For this purpose let us consider two other Fourier functions, F_u and F_v, derived in a linear manner from the given functions, F_α, F_β, F_γ, in accordance with the equations

$$F_u = \begin{vmatrix} F_\alpha & F_\beta \\ a & b \end{vmatrix} \quad \text{and} \quad F_v = \begin{vmatrix} F_\alpha & F_\gamma \\ a & c, \end{vmatrix}$$

where a, b, and c are scalars determined from the mean products,

$$a = \overline{F_\alpha F_\alpha},$$
$$b = \overline{F_\alpha F_\beta},$$
$$c = \overline{F_\alpha F_\gamma}.$$

It will then be seen that

$$\overline{F_\alpha F_u} = 0 \quad \text{and} \quad \overline{F_\alpha F_v} = 0,$$

and whatever F_u and F_v may be there is a real angle, ϕ, for which

$$\overline{F_u F_v} = \overline{\overline{F_u} \times \overline{F_v} \times \cos\phi},$$

since the product $\overline{F_u F_v}$ must be less than $\overline{\overline{F_u} \times \overline{F_v}}$.

If, therefore, we take two planes inclined to each other at an angle, ϕ, and take two vectors, u, v, in these planes, drawn perpendicular to the line of intersection, and of such length that

$$\overline{\overline{u}} = \overline{\overline{F_u}} \quad \text{and} \quad \overline{\overline{v}} = \overline{\overline{F_v}},$$

and if, in addition, we choose a vector, α, along the line of intersection of these planes, and of such length that

$$\overline{\overline{\alpha}} = \overline{\overline{F_\alpha}},$$

we shall then have

$$\overline{\alpha u} = 0 = \overline{F_\alpha F_u}$$

$$\overline{\alpha v} = 0 = \overline{F_\alpha F_v}$$

$$\overline{uv} = \overline{\overline{u} \times \overline{v} \times \cos\phi} = \overline{\overline{F_u} \times \overline{F_v} \times \cos\phi} = \overline{F_u F_v},$$

or the three vectors α, u, v completely represent the series F_α, F_u, F_v.

Since β and γ are linear functions of α, u, and v, while F_β, F_γ are the same linear functions of F_α, F_u, and F_v, it follows from the foregoing reasoning that β, γ also represent the periodic functions F_β, F_γ.

It is thus established that:—

Any three periodic functions can be represented by vectors in such a way that the length of the vector represents the magnitude of the function, while the scalar product of any two vectors represents the mean value of the product of the corresponding functions. Any other periodic function derivable from the original three, in a linear manner, can be similarly represented by a vector, derivable from the original three vectors by means of the same linear relation.

It will, however, be seen that it is impossible, in three-dimensioned space, to find vectors to represent four arbitrary periodic functions, unless a linear relation connects them. For instance, the functions $\sin pt$, $\cos pt$, $\sin 2pt$, $\cos 2pt$ cannot be so represented. The mean product of any two of these is zero, and it is impossible in three-dimensioned space to find four lines each of which is at right angles to the other three.

In discussing problems the solutions of which are expressible in terms of the mean square of a Fourier function, or the mean product of two such functions, it may sometimes prove advantageous to make use of the foregoing theorem. As a rule the complicated Fourier series occurring in actual problems are obtained in an indirect and somewhat artificial manner by expressing in precise mathematical language the value of some periodic quantity which can be represented graphically in a very simple way. In some cases the curve, though perfectly determinate, follows no recognised mathematical law; in other cases it consists of a series of discontinuous curves, each of simple mathematical character. In all such instances the mean square of the ordinate of such a curve, and the mean product of the corresponding ordinates of two such curves, can more easily and quickly be obtained than the Fourier expansions themselves. If the problem only demands a knowledge of these mean squares and mean products, the vector method will probably be the simplest one

to apply, and possibly many problems exist which may be solved in this manner. This seems the more likely when it is remembered that, in problems involving periodic forces and fluxes, the *energy* depends upon the *mean product of the force and its corresponding flux*, or, if the forces and fluxes are proportional, upon the *mean square* of the value of either. The theorem, however, seems most immediately applicable to the case of alternate current problems.

To apply the theorem to alternating currents, let us first of all consider the case of a network of conductors in which there are only three arbitrary periodic electromotive forces, E_1, E_2, and E_3, acting in the branches. If the resistances are all non-inductive, it will always then be possible to express the current in each branch, and the potential between any two points, as a linear function of E_1, E_2, and E_3, the coefficients only involving the resistances of the network branches. If α_1, α_2, and α_3 are vectors representing the periodic functions E_1, E_2, E_3, it will be possible to represent all the potentials and all the currents involved by means of a network of vectors.

A case in which the resistance of one of the branches is inductive, owing to its possession of a constant coefficient of self-induction, can best be treated by assuming that such a branch possesses an arbitrary electromotive force E, related to the current C, passing through the branch in a manner given by the equation

$$E = L\frac{dC}{dt}.$$

We know nothing of the relation between the vectors representing E and C, except that their scalar product is zero, and they are in consequence perpendicular to each other. No energy is wasted, due to self-induction, since

$$\int_0^T CL\frac{dC}{dt}\,dt = 0.$$

This relation gives a certain connexion between the coefficients which express C as a linear function of the vectors denoting the electromotive forces, but does not make it possible to express E as a linear function of the other electromotive forces E_1 and E_2. For although we may put $E = \lambda_1 E_1 + \lambda_2 E_2$, the coefficients λ_1 and λ_2 will not be scalar quantities, since they will involve not only the resistances of the branches of the network, but also the operator d/dt. It will always be possible to find a vector to represent E, but only in special cases will this vector be in the same plane as the vectors denoting E_1 and E_2.

Similarly a case in which two points of the network are connected with the terminals of a condenser of capacity K can best be treated

by assuming that the condenser is replaced by a conductor possessing an arbitrary electromotive force E, related to the current passing through the branch in a manner given by the equation

$$E = -\frac{1}{K}\int_0^t C dt.$$

The vectors representing E and C are such that their scalar product is zero, and the same remarks apply as in the preceding case.

The case of a transformer can be similarly treated. The reaction due to the varying induction of the core produced by a current C through the primary, must be treated as an arbitrary electromotive force E. The case will differ from that of constant self-induction, in that the vectors representing E and C are at right angles in the latter case, but not in the former, since, owing to hysteresis and eddy currents, the core absorbs energy, and the scalar product of the vectors E and C cannot be zero.

It will thus be seen that *the effect of inductance in a branch of the network is the same as that of an arbitrary electromotive force placed there, whether the inductance be due to constant self-induction, to condenser action, or to transformer action.* With this understanding it follows that *a vector figure can be drawn in space, completely representing the alternating currents and potentials in a network of conductors, provided the number of independent alternating electromotive forces does not exceed three. This figure can be drawn in a plane if the number of such electromotive forces is not more than two.* Of course, where a linear relation, with scalar coefficients, connects three electromotive forces, only two of these are to be regarded as independent.

Now the vector method which has hitherto been so largely used in explaining alternating current phenomena, will not be really serviceable in the new and extended application here suggested, except in cases in which the vector figure is either confined to one plane, or can, with all essential accuracy, be treated as if that were the case.

Fortunately, several circumstances conspire to simplify the application of the theorem to alternate current problems. In the first place the vectors which are needed are divisible naturally into two sets: one representing the currents, and the other the voltages. It is never necessary to find the product of two of these vectors unless they belong to different sets. Although in some cases it may not be possible to construct the complete vector figure in one plane, it may still be possible to construct two figures—one for each set. In practical cases one of these figures (say the current vectors) will almost certainly be confined to one plane, and if the vectors of the other (or voltage) network be projected perpendicularly on to this plane, the product of any projected voltage vector and the appropriate current

vector will accurately represent the corresponding quantity of power, since it is a well-known property of vectors that the scalar product of any two vectors is the same as the scalar product of one of them and the perpendicular projection of the other on any plane containing the first.

It will only rarely happen that a plane figure will fail to give a sufficiently accurate representation of the relations between the volts and amperes in alternate current circuits. These circuits are generally simple in character. Complication is not caused by a multitude of conductors unless some of these are inductive. A network of non-inductive resistances may involve a large number of currents and potentials, but they are all connected by linear relations, and the construction of the vector network presents no difficulty. Wherever a transformer is used, the primary and secondary circuits can be treated as quite distinct. The vector figures representing them can indeed be drawn to different scales, the only necessary connexion between them being the relation which connects the power put into the primary core of the transformer with that taken from the secondary. As a type of the most complicated circuit likely to occur, we may take the case of an alternator feeding the primary coil of a transformer, the primary terminals of which are also connected through a condenser. Here we have three inductive circuits in parallel. There are only three currents to consider. One of these can be expressed as the sum of the other two, and hence the current vectors lie in a plane. There is only one voltage. This may not lie in the plane of the currents, but its projection on this plane will give all that is needed. Let E be the electromotive force of the dynamo, R the resistance of the armature and leads up to the terminals of the transformer, kept at voltage V, and let C be the armature current, we then have

$$E = RC + V.$$

The vectors representing E and V may not be in the plane of the currents, but their projections on this plane are vectors which are connected with each other by the same linear relation as in the above equation, and in nearly all practical cases the projections may be taken to represent the voltages.

The chief purpose for which this vector method has hitherto been used is to explain or to predict the nature of the changes caused in alternate current circuits by variations in resistance, self-induction, or capacity, and to deduce the relations which must be fulfilled in order that certain results may be obtained. It has been possible to do this because from the network representing one particular case it has been easy to draw the figures representing other cases by merely increasing the length of certain of the lines in the proper proportion.

It will be evident that the same process is equally applicable when the currents are no longer of a simple sinuous character, provided only that the network representing one particular case can be properly drawn; for, however complicated the network may become when it represents several such cases, it is obtained from the original network by increasing some of the vectors to correspond with the changes in the conductors. If we have a current, C, passing through (1) a wire of resistance R, (2) a coil of self-induction L, or (3) a condenser of capacity K, the voltages at the ends of these conductors are given respectively by the expressions

$$RC,\ L\frac{dC}{dt},\quad \text{and}\quad -\frac{1}{K}\int_0^t C\,dt,$$

and hence the vectors representing them are proportional respectively to

$$R,\ L,\ \text{and}\ 1/K,$$

and the effect of an alteration in R, L, or K can generally be easily indicated on the network figure.

It may, therefore, be taken as established that the vector method of representing alternate current quantities is as applicable when the currents and potentials vary in a manner widely different from the sine law as when they are simply sinuous. But it is desirable in this connexion to repeat and emphasise an old warning about the continued use of coefficients of self and mutual induction in the theoretical investigations of alternate current problems. These coefficients may be of service in connexion with coils not possessing iron cores, but they are very misleading in commercial cases in which iron is present. By no choice of the value of these coefficients is it possible to represent the phenomena of hysteresis. The presence of the latter implies that the current has a different law of variation from that of the voltage, while the assumption of constant induction coefficients necessitates that if either the current or potential follows a sine law the other must also. Again, constant self-induction involves the assumption that there is no loss of energy in the magnetic changes going on in the iron, an assumption which is not only known to be wrong, but one which involves an error of serious practical importance. It has already* been pointed out that the difficulty of reconciling theoretical formulæ with experimental results does not arise so much from assuming that the currents vary according to a simple sine law, as from the quite unjustifiable assumption and use of constant coefficients of induction.

* See 'Electrician,' June 5 1891, "Alternate Current Theory," by Dr. W. E. Sumpner.

"Note on recent Investigations on the Mechanical Equivalent of Heat." By E. H. GRIFFITHS, F.R.S. Received and Read June 17, 1897.

Yesterday (June 16) I received, from Professor Ames, of Johns Hopkins University, the results of an important investigation which has been carried out under the direction of Professor Rowland and himself. An elaborate comparison of the thermometers used by Rowland, with certain standards supplied by the International Bureau, has been performed by Mr. W. S. Day. This comparison has been made under conditions as similar as possible to those prevalent when the thermometers were used by Rowland during his original experiments. A further most elaborate comparison has also been conducted by Messrs. C. W. Waidner and F. Mallory between a platinum thermometer, previously standardised at Cambridge (England), and Rowland's thermometers. The results of these separate investigations may be regarded as in practical agreement.

The conclusions to which this prolonged investigation has led the authors are so important that I take this opportunity of bringing them before the notice of the Royal Society. Full details will be given when the respective papers are published in America, but in the meantime the authors have given me permission to communicate their results to this Society.

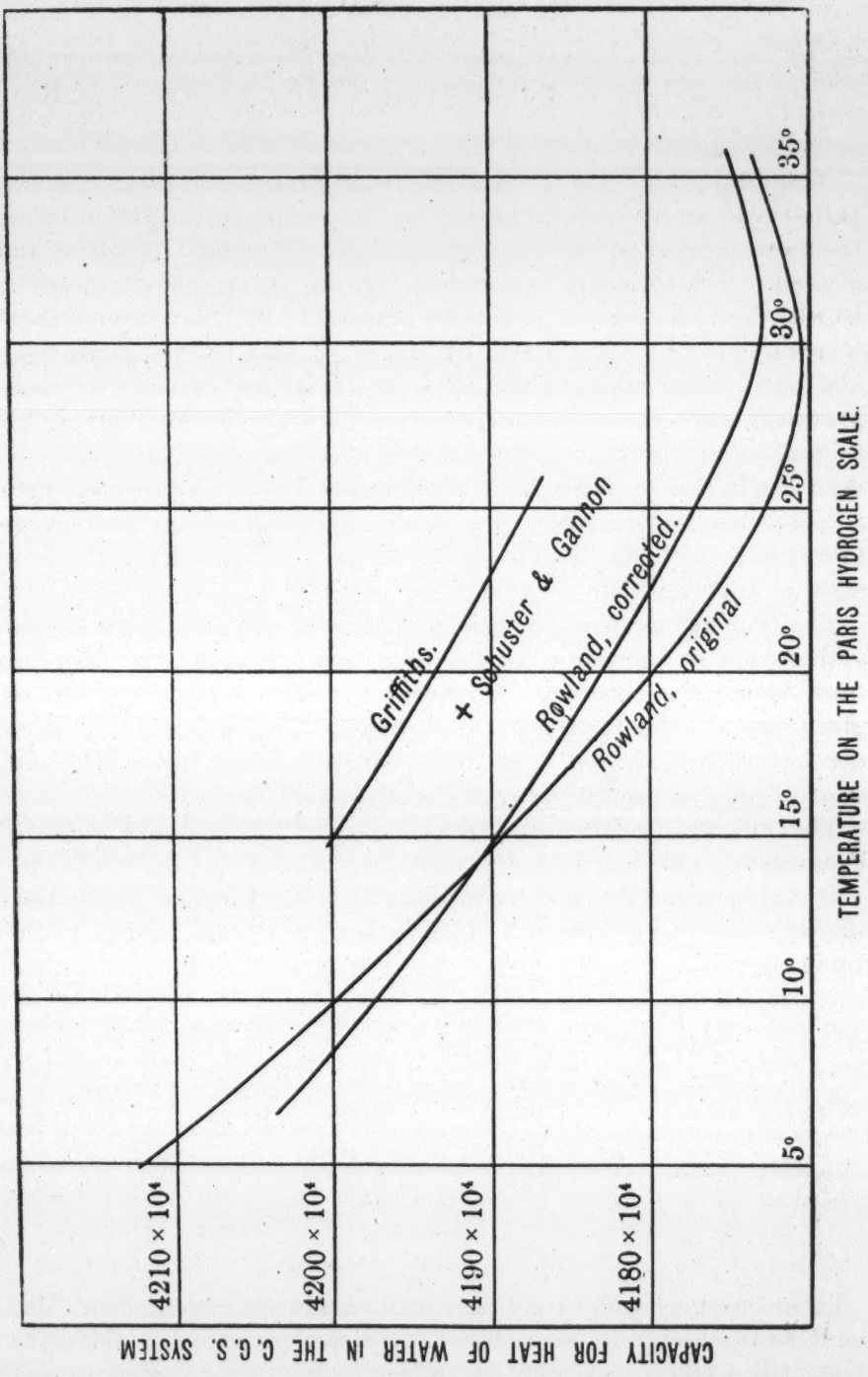
The following chart is a reduced copy of the one forwarded by Professor Ames.

It will be seen that the alteration in Rowland's values is considerable at certain temperatures. This is rendered more evident in the following table.

Temp.	Old.	Corrected.	Griffiths.	Schuster and Gannon.
6°	4209×10^4	4204×10^4		
10	4200	4197		
15	4189	4189	$4199{\cdot}7 \times 10^4$	
20	4179	4183	4193·2	4191×10^4
25	4173	4177	4187·4	
30	4171	4173		
35	4173	4174		

These numbers are in the C.G.S. system and hydrogen scale. The point to which I now chiefly wish to direct attention is this—the slant of Rowland's corrected curve is almost exactly similar to that resulting from my own experiments. Hence the uncertainties previously existing as to the changes in the capacity for heat of water over the range 10° to 25° C. are greatly diminished.

Mr. W. S. Day writes as follows:—



"If, therefore, curves were drawn representing the specific heat of water between 15° and 25°, as given by Rowland's experiments and by Griffiths', taking the value found in each case at 15° as unity, the two curves would be practically identical. This seems to show that

the difference between the value of the mechanical equivalent found by Rowland, and that found by Griffiths and by Schuster and Gannon, using electrical methods, may not be due to an error in the experiment itself, but to some error in the electrical standards of resistance or of electromotive force."

A happy result of this agreement (arrived at by such devious paths) concerning the changes in the capacity for heat of water, is that we can now utilise the results recently published by Professor Reynolds regarding the capacity for heat of water over the range 0° to 100° C. A hurried calculation indicates that if we accept Rowland's corrected values, the mean thermal unit over the range 0° to 100° is in close equality with the "thermal unit at 19°."

"Cathode Rays and some Analogous Rays." By SILVANUS P. THOMPSON, D.Sc., F.R.S. Received May 10,—Read June 17, 1897.

(Abstract.)

1. The size of the cathodic shadow of an object depends upon its own electric state, as already found by Crookes.* If it is negatively electrified the shadow expands. If it is positively electrified the shadow contracts. The position, as well as the size of a cathodic shadow, may be affected electrostatically; the rays which cast the shadow being repelled from a neighbouring body if the latter is negatively electrified. In some cases the contraction of the shadow of a narrow object that is made positively electrical (anodic) may go so far that the luminous margins approach and even overlap, giving the appearance of a bright or negative shadow in place of a dark one. The enlargement of a shadow when the object is made cathodic, and the diminution of the shadow when the object is made anodic, both depend upon the degree of exhaustion of the tube; and both are augmented up to a certain point by raising the degree of exhaustion. They are also unequal, the enlargement when the object is made cathodic vastly surpassing the diminution when the object is made anodic, other things remaining equal. The conclusion is reached that cathode rays are capable of being deflected electrostatically, being apparently strongly repelled from a neighbouring cathodic surface; and less strongly attracted towards a neighbouring anode. Incidentally it was observed that two cathode beams from two small disk cathodes can cross through or penetrate one another without interfering with one another.

2. The electrostatic deflection of cathode rays by an electrified object is found to be dependent upon the surface of that object as

* 'Phil. Trans.,' 1879, Part II, p. 648.

to whether it is a conductor or not. Objects protected by a non-conducting layer of glass do not at moderately low exhaustions, when made cathodic, repel or deflect cathode rays, and their shadow does not enlarge. But at a certain minimum exhaustion they suddenly exert an electrostatic deflection. Naked objects made cathodic deflect the cathode rays at all exhaustions.

3. The "splash" phenomenon often observed on the tube-wall of a Crookes' tube, where it is struck by the cathode beam, at the stage of exhaustion a little below that which suffices to evoke Röntgen's rays is due to electrostatic deflections of the cathode rays by the charges on the glass.

4. A hot wire used as an object casts a cathodic shadow precisely like that of the same wire cold. Under some circumstances, if the wire is heated by an electric current, the difference between the electrostatic state of its different parts may slightly affect the size of the shadow it casts.

5. Cathode rays cannot be concentrated by reflection either from a non-conducting or a conducting surface, nor by passage through a metal tube which is itself negatively electrified.

6. When cathode rays strike upon an internal metal target or anti-cathode there are emitted from the latter (both at exhaustions lower than suffice to produce Röntgen rays, and at exhaustions at which those rays are also produced) some internal rays resembling ordinary cathode rays in the following respects:—They produce a similar luminescence of the glass; they cast shadows of objects; they are susceptible of deflection both magnetically and electrostatically. But they produce no Röntgen rays where they fall upon the glass surface. They do not follow either the law of specular reflection, nor that of diffuse reflection, but are emitted from the anti-cathode surface apparently according to a similarly anomalous distribution to Röntgen's rays, *i.e.*, with nearly equal intensity, at all angles up to 90° with the normal. It is proposed to call these rays para-cathodic rays in contradistinction to the ordinary or ortho-cathodic rays. From the similarity of their distribution with that of the Röntgen rays it is inferred that the physical processes concerned in their production are identical. These para-cathodic rays are emitted from the anti-cathode both when the latter is made an anode, and when it is neutral or even made cathodic. From an anti-cathode there may proceed at one and the same time, and in one and the same direction, para-cathodic rays and Röntgen rays, which, meeting an interposed object, may cast simultaneously two shadows—a para-cathodic shadow on the glass, and a Röntgen shadow on an external screen of barium platinocyanide. The former shadow can be deflected by a magnet, the latter cannot. The former shadow expands if the object is made cathodic; the latter does not.

7. If thin metal screens are used to sift the cathode rays the luminescent phenomena change. The rays of least penetrating power appear to be most susceptible to magnetic and electrostatic forces. The various constituents of a heterogeneous cathode beam are emitted in various proportions at different degrees of exhaustion. In the cathode rays emitted at higher degrees of exhaustion there is a greater proportion of the less-deflectable rays. The least-deflectable rays are those which most readily penetrate through a perforated screen when that screen is itself made cathodic.

When ordinary cathode rays fall upon a perforated screen which is itself made cathodic, or are attempted to be passed through a tubular cathode, there emerge beyond the screen or tube some rays, here termed dia-cathodic rays, which differ from the ortho-cathodic, and also from the para-cathodic rays. These dia-cathodic rays are not themselves directly deflected by a magnet. They show themselves as a pale blue cone or streak. Where they fall on the glass they do not excite the ordinary fluorescence of the glass. The dia-cathodic rays excite, however, a different or second kind of fluorescence; the tint in the case of soda-glass being a dark orange. Intervening objects in the beam or cone of dia-cathodic rays cast shadows. The orange fluorescence evoked on soda-glass by the dia-cathodic rays shows in the spectroscope the D lines of sodium only. The shadows cast by dia-cathodic rays are not deflected by the magnet, nor do they change their size when the object is electrified.

"Electrification of Air, of Vapour of Water, and of other Gases."

By Lord Kelvin, G.C.V.O., F.R.S., Magnus Maclean, D.Sc., F.R.S.E., and Alexander Galt, B.Sc., F.R.S.E.

Received and Read June 17, 1897.

(Abstract.)

§ 1. In this paper the authors describe a long series of experiments on the electrification of air and other gases with which they have been occupied from May, 1894, up to the present time (June, 1897). Some results of earlier experiments, and of preliminary efforts to find convenient methods of investigation, have from time to time been communicated to the Royal Society, the British Association, and the Glasgow Philosophical Society.*

* "On the Electrification of Air," by Lord Kelvin and Magnus Maclean, 'Roy. Soc. Proc.,' May 31, 1894, and 'Phil. Mag.,' 1894; "Preliminary Experiments to find if Subtraction of Water from Air electrifies it," by Lord Kelvin, Magnus Maclean, and Alexander Galt, 'Brit. Assoc. Report,' 1894, p. 554; "Electrification of Air and other Gases by bubbling through Water and other Liquids," by Lord Kelvin, Magnus Maclean, and Alexander Galt, 'Roy. Soc. Proc.,' February, 1895;

§ 2. The method for testing the electrification of air which was used in their earliest experiments was an application of the water-dropper,* long well known in the ordinary observation of atmospheric electricity. Its use by Maclean and Goto† in 1890 led to an interesting discovery, that air in an enclosed vessel, previously non-electrified, becomes electrified by a jet of water falling through it. An investigation of properties of matter concerned in this effect, related as it is to the "development of electricity in the breaking up of a liquid into drops" which had been discovered by Holmgren ‡ as early as 1873, and to the later investigations and discoveries described by Lenard,§ in his paper on the "Electricity of Waterfalls," forms the subject of §§ 25—37 of the present communication.

§ 3. The electrification of air by drops of water, breaking from a jet in it, or falling through it, or striking on the ground, or on water, or on metal below it, produces absolutely no practical disturbance of the electric potential measured by the water-dropper in its use for the observation of open air atmospheric electricity: but constitutes a serious objection to its application for investigating atmospheric electricity within doors, unless in a very large room or hall, and renders it altogether unsuitable for experimental investigations, such as those described in the present paper.

§ 4. The authors were, therefore, early led to abandon it: and, for testing the electrification of air, they used three different methods, one or other of which they found convenient in different cases.

Method 1. Observation of electrification of the substance receiving the electricity equal and opposite to that taken by air in any case of electrification of air.

Method 2. Observation of the electricity of an insulated metal vessel into which electrified air is introduced, or from which electrified air is removed.

"On the Diselectrification of Air," by Lord Kelvin, Magnus Maclean, and Alexander Galt, 'Roy. Soc. Proc.,' March, 1895; "On the Electrification of Air," by Lord Kelvin, 'Glasgow Phil. Soc. Proc.,' March, 1895; "On the Electrification and Diselectrification of Air and other Gases," by Lord Kelvin, Magnus Maclean, and Alexander Galt, 'Brit. Assoc. Report,' 1895, p. 630.

* Kelvin and Maclean, 'Roy. Soc. Proc.,' 1894, and Kelvin, Maclean, and Galt, 'Roy. Soc. Proc.,' February, 1895; 'Electrostatics and Magnetism,' § 262 (from 'Lit. and Phil. Soc. of Manchester Proc.,' October 18, 1859).

† "Electrification of Air by Water-jet," by Magnus Maclean and Makita Goto, 'Phil. Mag.,' August, 1890.

‡ "Sur le Développement d'Électricité à l'occasion de la Dissolution en Gouttes des Liquides," 'Kongl. Sv. Vet. Ak. Handl.,' vol. 11, No. 8, pp. 14—43 (pour l'an 1873).

§ "Ueber die Electricität der Wasserfälle," by P. Lenard, 'Annalen der Physik und Chemie,' 1892.

Method 3. Observation of the electricity taken out of air by the electric filter.

§ 5. Method 1, was used in the experiments described in their communication to the Royal Society of February, 1895, from which it was concluded that air and several other gases tried became electrified by blowing them in bubbles through water, and through solutions of various salts, acids, and alkalis, in water. This conclusion was verified for the case of common air and pure water by collecting, into a large reservoir over water, air which had been bubbled through pure water in a U-tube. The electrification of the air thus collected was tested by a water-dropper, taking the same potential as the air at the centre of the reservoir. It was thus proved that the electrification of the air was negative, as was to be expected from the positive electrification which the authors had found on insulated vessels containing water through which air had been bubbled.

§ 6. Method 2, was used in the first experiments described in the present paper (§§ 16—24) which were undertaken for the purpose of determining approximately in absolute measure the total quantity of electricity in a given mass of electrified air; and particularly for finding the greatest electrification which could be communicated to a large quantity of air by needle points supplied with electricity from an electric machine. The result thus found in § 23, $3{\cdot}7 \times 10^{-4}$ C.G.S. electrostatic, is the greatest electric density (quantity of electricity per cubic centimetre) which the authors have hitherto been able to measure in air electrified by electrified needle points. But by an electrified hydrogen flame a density of 22×10^{-4} C.G.S. electrostatic unit was obtained in air (§ 65).

§ 7. In all the other experiments described in the present paper, Method 3 was used; but probably the authors must return to Method 2 if, in future, they undertake further experiments to find the greatest electric density which they can measure in air or other gases.

"On the Change of Absorption produced by Fluorescence." By JOHN BURKE, B.A. (Dub.), Berkeley Fellow of the Owens College, Manchester. Communicated by Professor ARTHUR SCHUSTER, F.R.S. Received June 10,—Read June 17, 1897.

(Abstract.)

If a body, A, of some fluorescent substance, such as uranium glass, be transmitting light from a similar body, B, which is fluorescing, the amount of light transmitted by A from B seems quite different, according as A is fluorescing or not. There appears to

exist in a dilute solution of fluorescine or eosine a small difference, but a strong solution of either does not permit its fluorescent light to penetrate, except a very small thickness of the liquid, whether fluorescing or not.

Sulphate of quinine is too transparent to the luminous rays, so that even if a change did exist it could hardly be detected. The case is otherwise with uranium glass, in which the effect is well marked.

The compounds of uranium are remarkable for many characteristic properties connected with fluorescence, which they seem to exhibit more readily than most other bodies. The spectrum of the fluorescent light emitted by uranium glass shows certain maxima and minima, as was noticed by Stokes in 1852.

The experiments which have been carried out with a view to testing whether a change in the absorption is produced by fluorescence, have been almost exclusively upon uranium glass. Regarding it as the most suitable substance to experiment upon, a careful series of experiments have been made to determine whether the phenomenon really existed or not.

If we call α and β the fractions of the light from B transmitted by A, according as the latter is fluorescing or not, for uranium glass 1 cm. thick, the values of α in a particular set of experiments, in which eye determinations were taken, were as follows. Each experiment consisted of twenty observations.

Exp. I.....	$\alpha = 0{\cdot}54$
II.....	$= 0{\cdot}46$
III.....	$= 0{\cdot}51$
IV.....	$= 0{\cdot}36$

And for β—

Exp. I.....	$\beta = 0{\cdot}70$
II.....	$= 0{\cdot}84$
III.....	$= 0{\cdot}72$
IV.....	$= 0{\cdot}92$

The mean value of α obtained from these being $\alpha = 0{\cdot}47$, $\beta = 0{\cdot}79$.

The ratio $\frac{\beta}{1+\alpha}$ was also independently determined, and found as the mean of eighty observations to be 0·507.

The values of α and β have also been determined photographically, giving

$\alpha < 0{\cdot}48$	$\beta > 0{\cdot}75$
$> 0{\cdot}43$	$< 0{\cdot}89$.

If we take the maximum value of α given photographically 0·48, the

ratio $\frac{\alpha}{1+\alpha} = 0{\cdot}32$, and instead of obtaining equality when the photometer is adjusted for this value the difference is most marked.

The effect has also been shown by obtaining two photographs on the one plate; one photograph being the result of an exposure to the light from two fluorescing cubes one behind the other, and the second photograph the result of superposing the effect of the light from A alone, when fluorescing, upon that from B after having passed through A, when the latter was not fluorescing. The exposure in each of the three cases being the same, a very distinct difference is shown in the result; the superposed photographs being always the darker in the negative, notwithstanding the fact that the resultant effect of superposing two photographs due to light of the same intensity, or nearly so, has been found not to be equal to but less than that due to light of double the intensity acting for half the time. If the resultant effect were equal to the sum of the separate ones, the effect caused by the change of absorption would have been still more marked.

In the determinations of α and β a null method has been employed by which any appreciable want of uniformity in the illumination can be detected.

The source of illumination has been almost invariably the spark discharge of a Leyden jar between cadmium electrodes, being one of the richest sources of the fluorescence-exciting rays, and the photometer one specially constructed for the purpose.

"On the Theory of the Magneto-Optic Phenomena of Iron, Nickel, and Cobalt." By J. G. LEATHEM, B.A., Fellow of St. John's College, Cambridge. Communicated by Sir ROBERT S. BALL, F.R.S. Received May 11,—Read June 17, 1897.

(Abstract.)

In Mr. Larmor's Brit. Assoc. Report (1893) on the Action of Magnetism on Light, it is pointed out (§ 20) that there are two ways in which the magnetic field may affect the phenomena of light propagation, and two corresponding types of magneto-optic theory. It is the object of the present paper to take the fundamental equations of the second type of theory in a general form on the lines of Mr. Larmor's recent papers on Electrodynamics, and to develop them so as to obtain the solutions of the problems of magnetic reflection and of transmission through magnetised films; the formulæ so obtained are compared with the available experimental results, and the agreement of the theory with experiment thus put to the test.

2. The notation is nearly the same as Maxwell's, (P, Q, R), (u, v, w), (a, b, c), and K, having their usual significations; σ is specific conductivity, and c the velocity of radiation; (f'', g'', h'') corresponds to Maxwell's total electric displacement, and has components (f, g, h) and (f', g', h'), of which the former is the displacement involved in the ether strain, and the latter that involved in the polarisation of the matter. It being as usual assumed that for light oscillations the effective magnetic permeability is unity, the fundamental equations of the theory are as follows:—

(i) The two circuital relations,

$$\frac{dc}{dy}-\frac{db}{dz}=4\pi u \quad \text{and} \quad \frac{d\mathrm{R}}{dy}-\frac{d\mathrm{Q}}{dz}=-\frac{da}{dt}.$$

(ii) The equations of the current,

$$u=\sigma \mathrm{P}+g_3\mathrm{Q}-g_2\mathrm{R}+df''/dt,$$

where the vector (g_1, g_2, g_3) represents the Hall effect.

(iii) The displacement relations, and the elastic relations between electromotive force and the corresponding polarisation, viz.,

$$f''=f+f', \qquad f=\mathrm{P}/4\pi\mathrm{C}^2,$$

and

$$f'=\frac{\mathrm{K}-1}{4\pi\mathrm{C}^2}\mathrm{P}+b_3\frac{d\mathrm{Q}}{dt}-b_2\frac{d\mathrm{R}}{dt},$$

the vector (b_1, b_2, b_3) representing, in transparent matter, the whole magneto-optic effect.

3. The equations of propagation are found in the usual way, (u, v, w) being taken as independent variables. In them and in the boundary conditions (b_1, b_2, b_3) and (g_1, g_2, g_3) appear only as the vector, $\left\{\left(b_1\frac{d^2}{dt^2}+g_1\right), \left(b_2\frac{d^2}{dt^2}+g_2\right), \left(b_3\frac{d^2}{dt^2}+g_3\right)\right\}$, which is assumed equal to $\mathrm{C}_0e^{ix}(\alpha_0, \beta_0, \gamma_0)$, where ($\alpha_0$, β_0, γ_0) is the intensity of magnetisation; C_0e^{ix} is thus the single magneto-optic constant of the theory.

4. The principal experiments which have been used to test the theory are those of Drs. Sissingh and Zeeman on magnetic reflection, their observations being measurements of the phase m and amplitude μ of the "magneto-optic component" of the reflected light, for various angles of incidence. It is noteworthy that the value of m derived from the theory involves x, but not C_0; while the theoretical value of μ is proportional to C_0, but does not involve x: this circumstance makes the test a severe one.

5. The following table will serve to indicate what sort of agreement is found to exist between the theory and the experiments.

Equatorial Reflection from Iron.

Angle of incidence.	Observed value of m.	Calculated value of m.
86° 0′	209° 26′	272° 35′ $- x$
82° 30′	204° 22′	265° 19′ $- x$
76° 30′	194° 49′	256° 31′ $- x$
71° 25′	190° 3′	251° 13′ $- x$
61° 30′	181° 49′	244° 18′ $- x$
51° 22′	179° 0′	239° 45′ $- x$
36° 10′	174° 9′	235° 27′ $- x$

If we suppose that the value of x is about 62°, the agreement shewn is remarkably good. Experiments on polar reflection from iron point to almost exactly the same value for x.

If we suppose the value of C_0 to be given by

$$-C_0 = 7{\cdot}283 \times 10^{-11},$$

the ratios of the calculated to the observed values of μ for the above angles of incidence are found to be respectively 1·13, 0·96, 0·99, 0·97, 1·01, 1·03, and 0·97; so that in the case of the amplitudes also there is good agreement.

6. For nickel the agreement of theory with experiment, though not so exact as in the case of iron, is still very good. The values indicated for the constants are

$$x = 76° \text{ to } 80°, \qquad -C_0 = 9{\cdot}225 \times 10^{-12}.$$

The agreement is better for cobalt, being specially good in the amplitude experiments. For polar reflection at incidences of 45°, 60°, and 73°, the ratios of the calculated to the observed values of μ are found to be 0·98, 0·97, and 1·07 respectively. The values indicated for the constants are

$$x = 64°, \qquad -C_0 = 1{\cdot}227 \times 10^{-10}.$$

7. The theory gives a satisfactory account of a phenomenon which has only recently been discovered, namely an effect of the component of magnetisation perpendicular to the plane of incidence. The change of phase which measures this effect has been observed by Zeeman, who, in a particular case, found it to be $0{\cdot}003 \times 90°$, with a mean error of $0{\cdot}001 \times 90°$. The value indicated by the theory is $0{\cdot}00243 \times 90°$.

8. Two of the many available experiments on transmission through magnetised metallic films are used to test the theory. In one of these the rotation observed by Lobach is 1·62°, while the value indicated by the theory is 0·961°. In the other the rotation observed by

Drude is 4·25°, the theoretical value being 2·972°. When it is borne in mind that the value of the magneto-optic constant derived from reflection experiments has here been applied to test experiments on transmission through films, with results not only of the same order of magnitude, but identical within the limits of uncertainty of the intensity of magnetisation, the agreement must be considered as a very satisfactory vindication both of the theory and of the experiments.

9. It is to be noticed that, as b_1, b_2, b_3 are necessarily real, the imaginary part of $C_0 e^{ix}$ must be entirely accounted for by the Hall effect. Hence the present theory involves the supposition that the Hall effect is very much greater for exceedingly rapidly alternating currents than for steady ones. There is nothing unnatural in this supposition, which may be compared with the fact discovered by Maxwell, that the ordinary coefficients of conductivity are very much smaller in the optical circumstances.

"Magnetic Properties of Iron at High Temperatures." By J. Hopkinson, D.Sc., F.R.S. Received June 10,—Read June 17, 1897.

The present note is for the purpose of correcting two points in my paper in the 'Philosophical Transactions,' A, vol. 180.

First.—I was of opinion that my experiments showed that heating iron above its critical point did not entirely destroy the effects of previous magnetisation. Recent experiments I have made do not confirm this opinion. I would therefore wish to *delete* the following sentences in which the matter is referred to, viz.:—Page 414, lines 12 and 13; the first paragraph on page 454; the first two lines on page 455; the first paragraph on page 457; and from the words "two things" on line 5 to the word "second" on the same page. Also in the 'Proceedings of the Royal Society,' vol. 45, page 321, strike out the two paragraphs at the top of the page.

Second.—I have since been unable to obtain so great recalescence with approximately pure iron as is shown in Curve XXXIX, and can only conclude that I must have been in error as to the composition of the sample examined. I therefore wish in the last paragraph of the paper to speak of the sample as of unknown composition; to strike out the words "This shows why soft iron apparently does not recalesce"; to substitute "this" for "the" and strike out "of the soft iron" in the last line.

I also take the opportunity of correcting an error in the joint paper of myself and Mr. Wilson, vol. 189, pages 109—136. *Delete* the four

lines on page 121 from "The extent" to "0·001," and substitute "The capacity of window glass is but little affected by variations of frequency at ordinary temperatures." The statement as it stands is hardly likely to mislead as it is obviously incorrect; it is unlikely that capacity would increase with frequency.

"On the Distribution of Frequency (Variation and Correlation) of the Barometric Height at diverse Stations." By KARL PEARSON, M.A., F.R.S., University College, London, and Miss ALICE LEE, Bedford College. Received June 15,—Read June 17, 1897.

(Abstract.)

1. Although this paper contains the results of a very large amount of arithmetical work, which has been in progress during the last two or three years, it is not intended in the first place as a contribution to the meteorology of the British Isles. It is especially intended as an *illustration of method.* The authors believe that hitherto no exact theory of variation or of correlation has been applied to meteorological observations, and they have endeavoured to indicate that fruitful results may be obtained from such a theory when applied to one branch at least of meteorology, namely, barometric frequency. They wished to deal with a fairly extended area with an easily accessible material, and this was found in the *Meteorological Observations at Stations of the Second Order* for the British Isles. The "telegraph" stations would have provided better material, but it was far less accessible. The authors have accordingly only dealt with three telegraph stations. The main body of their data was drawn from twenty stations of the second order, four of which are in Ireland, and the remainder distributed round the coast of England, Wales, and Scotland, as indicated on a chart accompanying the memoir.

2. Their first object was to determine the nature of the barometric frequency distribution. By means of tables and plates it is shown that it can be described with a very high degree of accuracy by the use of a generalised frequency curve of the type—

$$y = y_0\left(1+\frac{x}{a}\right)^p e^{-\gamma x},$$

a type which has been fully discussed in a previous memoir on skew variation.

A standard frequency curve for the British Isles having been selected, it is shown that the frequency distribution varies con-

tinuously from this type as we pass from station to station, and appears to be fairly uniform along lines which are termed generalised isobars. If this result should be confirmed, then the positions of these generalised isobars would require a larger series of stations with observations for a longer period of years to determine them accurately.

3. The authors' next object was to discover what constants of the barometric frequency suffice to describe it with the least probable error. A somewhat elaborate investigation was accordingly made into the probable errors of the constants, and four *physical* quantities, the mean, the variation (or standard deviation), the skewness, and the modal frequency were found to be the constants, which described a local barometric frequency with the smallest probable errors.

4. They have next discussed the chief physical features of a barometric frequency distribution.

(*a*) The modal height and the modal frequency are found to possess certain advantages over the mean height and the mean frequency. Various methods are considered for approximately determining the position of the mode. The mean modal height for Great Britain is shown to be very nearly the standard atmosphere of 30″.

(*b*) The variability of the barometric height and the skewness of the distribution are discussed at some length. A formula is given in terms of the skewness for calculating the probability that the barometer at any station will be in excess or defect of the mode or of the mean.

(*c*) A test of the accuracy of the observations for the twenty coast stations is made by attempting to interpolate the frequency constants of London and Cambridge from those of Southampton, Hillington, and St. Leonards. The results are fairly close when tested with the results for London and Cambridge calculated directly from the telegraph station returns.

5. In the second part of the memoir the subject of correlation is dealt with. It is shown that within the limits of the British Isles there is a very high degree of correlation (as high as 0·9824 between Babbacombe and Churchstoke), only sinking to 0·7572 if we take practically the utmost reach of the British Isles. The correlation differs with directions more or less along and more or less perpendicular to the generalised isobars, and it is pointed out that this gradual change with direction and distance, combined with change owing to the interval of time between observations, enables the meteorologist to find systems of stations with almost every variety of correlation coefficient.

The probable height and probable deviation from that height at any given station, based on a knowledge of the heights, contempora-

neous or not, at one, two, three, or more other stations, are considered. It is indicated that with a proper arrangement of times and distribution of stations it must be possible to make the probable deviation zero or nearly zero, and hence to predict with very great accuracy the height at one station from a knowledge of heights at other selected stations. The suggestion is made that this principle might very possibly be applied to closely predict future barometric heights at a given station from antedated observations at other selected stations.

Various theorems are deduced from the general principles of correlation: thus, it is shown that—

(*a*) There is a balance height for every pair of stations, such that when the barometer stands above this height at one station it will usually stand below it at the other, and *vice versâ*.

(*b*) That for a very considerable number of triplets of stations which are positively correlated together, so that a high barometer at one means usually a high barometer at a second, it can still be predicted that if the barometer be steady at one, a rise or fall at the second denotes a fall or rise respectively at the third member of the triplet.

These principles are illustrated from actual observations at stations in the British Isles. The memoir concludes with an appendix, giving an account of a frequency-recording barometer devised by Mr. G. U. Yule.

The writers hope that their paper may draw attention to the importance of rendering the large amount of barometric observations now made, available for the easy calculation of the variation and correlation coefficients. They consider that if a chain of stations round a large continental area could have their correlation for a series of intervals of time worked out, much might be done in the way of very close prediction of barometric changes.

The authors have heartily to thank Mr. R. H. Scott, F.R.S., for assistance in a variety of ways.

OBITUARY NOTICES OF FELLOWS DECEASED.

Sir John Eric Erichsen, Bart., Surgeon Extraordinary to the Queen, who died on September 23, 1896, will, without doubt, take rank amongst the most distinguished surgeons of the century. There must be very few of the present generation of practitioners of surgery in English-speaking countries who have not come under his influence, at any rate indirectly; for his book on the Science and Art of Surgery, which made its first appearance in 1853, at once took a foremost place as the principal text-book of that subject, a place which in successive editions, to the number of ten, it has maintained ever since. Nor has it failed to be translated into most European languages, and even, it is said, into Chinese

Erichsen was descended, on the father's side, from a Danish family, whose importance is still testified in Copenhagen by the palace which bears the same name. On his mother's side he sprang from a Somersetshire stock—the Govetts. He was born on July 19, 1818, in Copenhagen, but was educated in England. After the termination of his school career, he entered as a student at University College, where Liston was then the chief surgeon, and where also he came under the strong personal influence of William Sharpey, who, as in so many other cases, early discovered the scientific bent in young Erichsen's character, and was instrumental in directing it into a useful field. Having, like many other professional men, more leisure in the early period of his career than he ever had in the later, Erichsen devoted himself to the study of physiology, and for a time taught the subject at the Westminster Hospital Medical School. He acted in 1844 as Secretary to the Section of Physiology of the British Association, and was the working member of a small committee appointed by the Association to undertake an experimental enquiry into the mechanism and effects of asphyxia, and the prevention and treatment of that condition. His researches on this subject were published in 1845, and his labours were recognised by his being awarded a gold medal, of the value of 50 guineas, by the Royal Humane Society.

In those days, no education in medicine and surgery was considered complete unless it had been supplemented by residence in Paris, and attendance upon the cliniques of the great French physicians and surgeons. Accordingly we find Erichsen pursuing his studies in that capital, and, amongst other things, present on the first occasion of the performance of a colotomy, by Amussat.

Erichsen had not for long leisure to devote to scientific pursuits, for, owing partly to his exceptional ability, partly to extraneous circumstances, his advancement in the profession of surgery was very rapid. Liston had died suddenly, from aneurism, in 1847, and his place was filled for a short period by Syme, who soon, however, returned to Edinburgh. Mr. Richard Quain then became surgeon to the hospital, and with him was associated as Professor of Surgery, Mr. Arnott, at that time senior surgeon to the Middlesex Hospital. The position of assistant surgeon, which was rendered vacant by Quain's advancement, was filled by the appointment of Erichsen. This position he only held for two years, for at the end of that time Arnott resigned his chair and the surgeoncy to University College Hospital, and Erichsen was appointed by the Council of the College to succeed him. This was in 1850, and at the early age of thirty-two. How abundantly the choice of the Council was justified is shown by the brilliant success which attended his teaching. It is not too much to say that the reputation of the School of Medicine at University College, as a training place for practical and scientific surgery, is due in no small measure to his influence.

Erichsen held the Professorship of Surgery for sixteen years, and then exchanged it for the Chair of Clinical Surgery, which he held for another nine years, so that he was for a quarter of a century occupying a leading position in the teaching of surgery in one of the first medical schools in the kingdom. Apart, therefore, from the indirect influence exerted by his writings, the direct influence he exerted in the training of students for the medical profession was considerable. And it must be recorded of him that, unlike many a brilliant surgeon of his time, he regarded, and taught his pupils to regard, surgery as a science to be studied rather than simply as an art to be displayed—a line of thought which has been pursued with conspicuous success by the most eminent of those pupils, the present President of this Society.

Although essentially an all-round surgeon, and one whose opinion in any branch of surgery was regarded, and rightly so, as of more than average weight, there were one or two branches to which Erichsen more especially devoted his attention. Early in his career he took up the subject of aneurism—always one of great interest to him—and contributed several articles dealing with its pathology and treatment. Later, he turned his attention to the important, and at the time ill-understood, subject of the results of railway injuries, a subject upon which he was for many years the acknowledged authority.

Erichsen possessed a singularly attractive personality, with a courteous and winning manner, which reflected the kindly nature within, and a dignified presence, without a tinge of pomposity. His

language, whether in public or private utterance, was always well chosen, his manner of speaking clear and to the point, his material well arranged. These qualities, although he possessed no particular gift of eloquence, made it always a pleasure to listen to him. A prominent characteristic of the man was his tact in dealing with difficult situations, and, it may be added, with difficult people. He was always ready, when appealed to, to afford assistance by advice, or even in a more material way, to his friends and professional brethren who might stand in need of such assistance, and many a young man has benefitted by his thoughtful liberality. Small wonder that he was highly regarded in the profession of which he was so distinguished an ornament, and by the colleagues and pupils with whom he was so long and so honourably associated. His eminence in surgery was acknowledged by the Royal College of Surgeons, in 1880, in electing him to occupy the presidential chair, and at the International Medical Congress, in 1881, in his selection to preside over the section of surgery at the meeting in London. He was also President for three years of the Royal Medical and Chirurgical Society. He was a member of the Royal Commission on Vivisection, which sat in 1875, and was the first Inspector for England and Scotland under the Act which resulted from the sittings of that Commission. He was elected a Fellow of this Society in 1876. He contested the Universities of Edinburgh and St. Andrews, in 1885, in the Liberal interest, but without success. He received the honorary degree of LL.D. from the University of Edinburgh, on the occasion of the tercentenary celebration, and was besides the recipient of numerous honours, culminating in the somewhat tardy recognition of his merits by the Government in creating him a baronet, in 1895. But the honour which he, perhaps, chiefly prized was that which his Alma Mater did him and herself in electing him, in 1887, to the important and dignified position of President of the Council of University College, a position which he occupied until his death, to the manifest advantage of both the College and the Hospital, with which his whole career had been so closely interwoven.

Erichsen was married, in 1842, to Mary Elizabeth, daughter of Captain Thomas Cole, R.N. They had no children. His wife died in 1893, and was buried at Hampstead Cemetery, and he was himself laid to rest in the same grave, on September 26, 1896.

E. A. S.

Samuel James Augustus Salter, or James Salter, as he always called himself, came from a medical family at Poole, various members of which achieved distinction in medicine or in science. He was born in 1825, and received his medical education at King's College. How well he spent his pupil days may be gleaned from

the fact that he graduated as M.B. at the London University, and acted as one of Fergusson's house-surgeons. His uncle, Mr. Thomas Bell, the eminent zoologist, was one of the few highly trained dental surgeons of his day, and induced his nephew to join him in partnership, as also in the conduct of the dental department of Guy's Hospital. The foundations of his fortunes were thus laid, to the detriment, it must be feared, of his work in science. Immersed as he was in practice he still found time to interest himself in scientific pursuits, and was an active member of several societies, medical and other.

He was also the author of a paper in the 'Philosophical Transactions' on the "Structure and Growth of the Tooth in Echinus," and in 1863 became a Fellow of the Royal Society. He had become a Fellow of the Linnean Society in 1853.

His work 'Dental Pathology and Surgery,' 1874, at once secured him a leading place, but the time expended in its preparation had been so great that it is to be feared that the work was hardly issued before it was out of date.*

Salter had a good general knowledge of botany, and it is to him that we owe the discovery of a fact the full significance of which is not worked out yet. This consisted in the discovery of perfectly formed pollen grains within the tissue of the nucellus of the ovule, as recorded and figured in the 'Transactions of the Linnean Society,' vol. 24, 1863, p. 143, pl. 24. Whether the megasporange was developed or not, we do not know, nor what were the precise cells engaged in forming pollen grains in this extraordinary position. It is to be hoped that some future investigator may be able to supply information on these points. That the condition is not confined to passion flowers we know from having seen a like state of affairs in *Rosa canina*.

On quitting his profession, Salter retired also from London life into the country, where he occupied himself with rural pursuits and horticulture, and amused himself with heraldry and archæology.

Personally, he was most amiable, and so stocked with general knowledge that he was a delightful companion and ready to afford help where help was needed. He married late in life, but his wife predeceased him by a year or two. He died March, 1897.

M. T. M.

* This, his only book, was rather the expansion of the writer's notes and experiences over a long series of years than a complete systematic treatise on the subject, and so had a kind of personal character. And although, after the manner of so many of the scientific writers of that day, he sometimes plunged into warm controversy, and not always, as things have turned out, on the right side; yet his work remains of solid value as a record of original and thoughtful observation, and as such is still frequently quoted in the text-books of to-day. C. S. T.

The death of Dr. George Harley, F.R.S. (on October 27, 1896), has removed in the midst of active labour a distinguished physician, a true lover of science, a devoted husband and father, and, to the writer of the present biography, an old and valued friend.

The subject of this notice was born at Haddington, on February 12, 1829, and came from a race of famed ecclesiastical and literary men. The Harleys of Haddington played an important part in the old Roman Catholic times, the Rev. Dr. John Harley, of Magdalen College, Oxford, becoming, in 1553, the first Protestant Bishop of Hereford, and his cousin, the Rev. William Harley, the first legally inducted Protestant clergyman in Scotland; both were pupils and friends of John Knox. After this time the Harleys of Haddington took to the law, and for a number of generations were lawyers of considerable local repute. George Harley's grandfather, Patrick Harley, born in 1717, was a lawyer, though better known locally as a poet and antiquarian. His father, born in 1766, lived as a country gentleman, at Haddington, but died when George was only three years old, leaving his education to his mother, who was a very talented woman, and her son inherited her abilities and originality.

George Harley was educated at the Haddington Burgh Schools, and at the age of 17 matriculated as a medical student at the University of Edinburgh.

While at Edinburgh Harley had every opportunity of acquiring the rudimentary knowledge of his profession, of which he duly availed himself. The Edinburgh University was then distinguished as a school of medicine by an unrivalled staff of Professors. John Goodsir was not only an accomplished human anatomist, but also learned in comparative anatomy, on which he frequently lectured, and the writer of this notice, who attended his lectures, together with George Harley, can testify to Goodsir's brilliant and fascinating delivery. Rather narrow-chested, thin, tall, and with intellectual features, Goodsir, speaking in a broad Scotch accent, would hold a large class in breathless interest on the history of the Medusæ, a subject on which he delighted to lecture. Allen Thomson lectured on physiology, in which he excelled. The microscope was not, however, at that time so intimately connected with the study of physiology as it was destined to become under Hughes Bennett, who succeeded Allen Thomson. In materia medica Dr. Christison, afterwards Sir Robert Christison, lectured to a large class, captivating students by his extensive knowledge of the subject he dealt with, and by his flowing and easy delivery. George Harley's eldest daughter afterwards married the nephew of Sir Robert Christison. In the Botanical Gardens, in spring and summer, Balfour would meet his class three or four times a week, and address his students with the impression that everybody ought to be a botanist, and

that a love of botany should be innate in every one of his audience. Alison, brother of the distinguished historian, was Professor of Medicine, a fine, rather stout and imposing gentleman, kindness itself to all the students, and painstaking in hospital work. It was reserved, however, to Hughes Bennett to introduce into clinical teaching the admirable method of examining patients which came into general use in the infirmary, and found its way by degrees into private practice. The chair of pathology was held by Dr. Henderson, while Dr. W. Gairdner, of Glasgow, perhaps the last of the survivors of the Edinburgh lecturers towards the end of the forties, held an extra-academical class of pathology, and delivered a set of well prepared and interesting lectures. In surgery Professor Symes and Professor Miller occupied the highest positions, Symes as an admirable operator, and Miller as an excellent lecturer, while Dr. James Simpson (afterwards Sir James) addressed several times a week a crowded audience on midwifery and gynæcology, subjects which particularly attracted George Harley's attention, as we find him acting as house surgeon to the Maternity Hospital; and it was through Sir James Simpson that Harley obtained his first appointment in London.

Such were the eminent professors under whose tuition George Harley acquired his earliest professional knowledge. But this was not all: the society at Edinburgh into which Harley and other students of medicine were admitted with true Scotch hospitality, contributed in no little degree to the development of his social and intellectual powers. Lord Jeffrey, the distinguished author of "Essays and Reviews," and other Judges, such as Lord Murray, Lord Ivory, and Lord Fullerton extended a friendly hand to many of the medical students, and asked them to dinner and evening parties.

While at Edinburgh, Harley became acquainted and made friends with many students, who were destined to become distinguished in after-life. Most of them have now departed from this world, amongst whom I might recall the names of Dr. Charles Murchison, who died at an early age—had he lived, he would undoubtedly have ranked amongst the most distinguished physicians of the time—and Dr. Spencer Cobbold, F.R.S., well known for his labours on Entozoa.

George Harley graduated at Edinburgh in 1850, at the same time as the writer of this notice, and was duly capped before a large audience. There were that year about 80 students who obtained their doctor's degree, while at the present time more than twice that number are yearly added to the roll of the Edinburgh graduates.

Before dismissing George Harley's career as a student, an indication of his promptitude and determination of character may be gathered from the circumstance that while acting as House Surgeon

to the Royal Maternity Hospital, he successfully performed the Cæsarean section on a woman who had just died, and extracted a living child, now grown up to be a father of a family. The case was read by Professor Simpson, at the Medico-Chirurgical Society of Edinburgh, and published in the 'Edinburgh Journal of Medical Science' for July, 1850.

After graduating, Dr. George Harley went to Paris, and there began his scientific studies. He entered the laboratory of Messrs. Wurtz and Verdeil, Rue Garanciére, and for a winter worked there, together with the author of the present notice. This laboratory had been opened by Professor Ad. Wurtz, the distinguished chemist. He had rented a spacious apartment on the ground floor of a house, Rue Garanciére, and at great expense and no little trouble, had turned it into a chemical laboratory, most complete in its fittings, where he was then joined by two other chemists, Dr. Verdeil, a Swiss gentleman, and Monsieur Charles Dollfus Galline, who was part owner of large dyeing works in Alsace, and had made a special study of the chemistry of colouring matters. Wurtz taught chemistry proper, Verdeil chemistry applied to physiology and pathology, and Dollfus chemistry in its application to technology.

The building enclosed a spacious courtyard, and on the side opposite to the laboratory, Charles Robin, who, with Claude Bernard and Majendie, was at the head of physiological science in France, opened a laboratory of experimental physiology, where a few Englishmen obtained instruction which in some instances matured into high distinction.

George Harley worked under Verdeil, and they soon became friends. Here he prepared all the constituents of the human body which can be obtained in the crystalline form, such as creatine, creatinine, sarco-lactates, urea, uric acid, hippuric acid, sodium taurocholate and glycocholate, fatty acids, &c.; he analysed biliary and vesical calculi, and indeed went through a complete course of physiological chemistry, but at that time he was mostly interested with the composition of urine.

That there was great activity in the Laboratory Rue Garanciére may be gathered from the circumstance that one of the students wishing to prepare creatine on a large scale, purchased an old horse, and walked the poor dilapidated beast into the laboratory, and it was shot then and there. What little flesh there was on it was cut off as quickly as possible, and submitted to the various operations required for the preparation of creatine; but little of the substance, however, was obtained.

Shortly after the author of this notice had left Paris he received from Harley a letter, which contained the following passage (the letter is dated 46, Rue Vaugirard, Paris, 26th June, 1852):—

"I am still working away at Verdeil's; he is at present making observations on the fats of the blood and fæces while I am working at urine. I have made a most important discovery, namely: that the colouring matter of the urine is hæmatin, which appears to have all the properties of that in the blood, and I have clearly shown the presence of iron combined with the colouring matter, and seemingly in the same manner as it is combined in the blood. I have found this substance in the urine of many pigs, horses, cows, &c., and can get it by three or four different processes."

Harley was in Paris at the time of the *Coup d'Etat* provoked by Louis Napoleon. The writer of this notice sallied forth on the morning of that memorable day with George Harley and E. Risler, a gentleman also at work in the Rue Garanciére, and who subsequently rose to a high position in France; he is now "Directeur de l'Institut Agronomique" in Paris. Paris was in a state of feverish excitement, the Place du Carrousel was so crowded that it was impossible to walk across it, when suddenly a lane opened in the crowd, and Louis Napoleon appeared on horseback, walking leisurely at the head of his staff. Thus it was that he rode round Paris, and the courage he exhibited won the day for him. Along the boulevards the cavalry was drawn up apparently from the Madeleine to the Bastille, and there was firing on the people. After the firing had ceased the party, which had been joined by Verdeil, thought of visiting the Boulevards. At the corner of the Rue Vivienne they came across a dead body on the pavement; a little further on there were two more; then a poor man who had been killed still grasping with his contracted fingers a coloured handkerchief, containing apparently clothes for the wash. The cavalry looked ominously threatening, drawn up on the opposite side of the street; one of them riding up enquired the object of the party on the boulevard, and then enjoined it to leave as soon as possible, as orders had been given to fire on anybody walking about the street. Of course his injunction was strictly complied with.

Harley's next paper after the publication of that on the presence of iron in the blood, was entitled "Recherches sur la physiologie du Diabète—nouvelle méthode pour produire artificiellement le diabète sur les animaux," 'Compt. Rend. Soc. Biol.,' 1853. In this year he was elected President of the Parisian Medical Society.

While in Paris Harley worked also under Majendie and Claude Bernard. Majendie was then giving up work, after having earned a deservedly great reputation, and was succeeded at the Collège de France by Claude Bernard, who delivered an admirable course of lectures on physiology which was attended by many English visitors and by Harley amongst others.

After two years' residence in Paris, Harley went to Germany, and

studied at the universities of Würzburg, Berlin, Vienna, and Heidelberg. A paper he published in German obtained for him the corresponding membership of the Physico-medical Society of the Würzburg University.

He returned to London at the end of 1855, and through the influence of Professor Sharpey, obtained the post of Curator to the Anatomical Museum of University College. In 1859 he was appointed to the Professorship of Medical Jurisprudence, and not long afterwards joined the Medical Staff of University College Hospital.

George Harley married, in 1861, Emma Jessie, the youngest daughter of James Muspratt, of Seaforth Hall, Liverpool. Among his children are Ethel, now Mrs. A. Tweedie, the known writer, and Dr. Vaughan Harley who, following in his father's footsteps, is now Professor of Pathological Chemistry at University College.

In 1861 Harley received the triennial prize of the Royal College of Surgeons for an Essay on the Anatomy and Physiology of the Supra-Renal Bodies, and the College also granted him the John Hunter Gold Medal, of which there has only been one awarded since 1858. In 1864 he was elected a Fellow of the Royal College of Physicians of London, and in the year following, a Fellow of the Royal Society, having previously been made a Corresponding Member of the Academy of Science of Bavaria, and of the Academy of Medicine of Madrid.

Harley's career was nearly cut short in 1864, while working with a powerfully illuminated small-lens microscope, when a blood-vessel burst in his left eye, ending in retinitis, which was so severe that he was confined to complete darkness for a period of nine months, ending, however, in a perfect cure. He had to wear dark-tinted glasses for a considerable time before he could use his sight freely, and remarked to the writer of this notice, that while wearing "goggles," as he could not see well, he became accustomed to look at things more carefully than he had done before, and that when subsequently he dropped his glasses, could actually see better than before, as from training he had improved his powers of observation. Other men under similar circumstances would probably have retired into a quiet private life, but the fact that Harley had sufficient self-possession to accept a complete privation of light for nine months shows the dogged resolution and perseverance which was, perhaps, the main trait of his character, and carried him through so much good and useful work.

Harley loved to roam over a variety of subjects; he would ponder over any matter of interest to him and then record his ideas in print.

Claude Bernard's lectures, which dealt with the physiological

action of poisons and the functions of the liver, were apparently the origin of Harley's scientific bent of mind, and we find him early in his career investigating the action of "atropine" in dilating the pupil, of strychnine on the spinal cord, and of the "Calabar bean." Then he took to the special study of the liver, and in 1883 wrote his elaborate treatise on diseases of the liver, a book which was very favourably received and had a great circulation, being translated into several languages. He communicated many papers to the Royal Society—one of them even as early as 1856–7, and in 1865 was the author of a paper in the 'Philosophical Transactions of the Royal Society' "On the Influence of Physical agents on the Blood, with Special Reference to the Mutual Action of the Blood on the Respiratory Gases." He also wrote in the 'Transactions' of the Microscopical, Zoological, and Anthropological Societies, in the 'Comptes Rendus de l'Académie,' and was the author of a number of contributions to the medical papers. It would be too long to give an enumeration of all George Harley's writings scientific and medical; he was never at rest; and when he died he must have felt conscious that he had done his duty and completed his work.

W. M.

Augustus Wollaston Franks was the elder son of Captain Frederick Franks, R.N., and Frederica, daughter of Sir John Sebright, Bart., of Beechwood, Herts. He was born on March 20, 1826, at Geneva, and his second name records the fact that the celebrated Dr. Wollaston, F.R.S., was one of his godfathers. At Geneva and at Rome his parents resided during his boyhood, when he became wel versed in French and Italian. His school education was, however, at Eton, whence in due course he proceeded to Trinity College, Cambridge, where he took his B.A. degree in 1849, and his M.A. in 1852. While still at college he exhibited a strong attachment to mediæval antiquities, and in 1849 published a volume on 'Ornamental Glazing Quarries,' many of the illustrations in which were drawn by himself. He subsequently published a treatise on 'Vitreous Art in the Art Treasures of the Manchester Exhibition.' It was, however, as one of the editors of Kemble's 'Horæ Ferales,' which was published in 1863, that he established his position as a scientific archæologist, for it was in that work that he first brought before the world his views on a phase of civilization corresponding in the main with what on the Continent is known as "the Early Iron Age," the remains of which are perhaps more characteristically present in the British Isles than in any other part of Europe. For the term "late-Celtic Period," by which this stage in human progress is now generally known, we are indebted to Sir Wollaston Franks.

He also took a warm interest in prehistoric archæology, and at

the time when Lartet and Christy were carrying on their investigations in the early caverns of the Dordogne he materially aided them in their researches, and also afforded valuable assistance in the publication of the 'Reliquicæ Aquitanicæ.' It was mainly through his friendship with Mr. Henry Christy that the latter resolved to bequeath his fine ethnological collections, together with an endowment fund, to trustees, of whom Mr. Franks was one. Through his exertions, and aided by his own liberality, the Christy collection has attained its present extreme importance, and has been incorporated with the national collections at the British Museum, where the whole have been arranged under the immediate superintendence of Sir Wollaston Franks.

His connexion with that Museum extended over a period of more than forty-six years. In 1850 an Exhibition of Mediæval Art was held at the rooms of the Society of Arts,. for which he acted as honorary secretary, and it would seem that the acquaintance that he then showed with all that related to mediæval antiquity induced Mr. Hawkins, the keeper of the Department of Antiquities at the British Museum, to offer to him the post of an assistant in that Department. On the 1st of March, 1851, he received the appointment, and during his long connexion with the Museum witnessed an immense development in its archæological collections, towards which he in no small degree contributed, and also a subdivision of the old Department of Antiquities into four departments, each charged with some special branch of archæology, of one of which, that of British and Mediæval Antiquities and Ethnography, he became keeper in 1866. To any one looking back to what the collections were at that date, and comparing them with what they are at the present time, the advance in extent, value, interest, and importance seems almost incredible. Much of this has been due to Mr. Franks' great personal influence with collectors, several of whom, actuated in no small degree by the confidence they reposed in him, have bequeathed their whole collections to the nation, while others have presented choice and valuable specimens. Of such liberality, he was himself the most conspicuous example. During the whole time that he was connected with the Museum it may be said that not a year passed without his presenting to the collections some objects for which the trustees had to return their special thanks, and when from want of funds some desirable purchase could not be made by the Museum, his private liberality was sure to come to the rescue. Nor was this liberality checked by the grave, for he has bequeathed to the Museum collections of rings, jewellery, drinking cups, and other objects, valued at a sum of £50,000.

In connexion with his work at the Museum may be mentioned the 'Medallic Illustrations of the History of Great Britain and

Ireland,' which was published in two volumes by the trustees in 1885. This book, which was based on a catalogue compiled by the late Mr. Edward Hawkins, was completed and in a great measure re-written by Mr. Franks, though the work was in reality outside his own department. In carrying out his object he was much aided by Mr. H. A. Grueber, and the 'Illustrations' will long remain the standard authority on the subject of British medals struck before the death of George II.

His connexion with the Society of Antiquaries was almost as long as that with the British Museum, he having been elected a Fellow in 1853. He did much to improve its library and its publications, and for upwards of twenty years he was Director of the Society. His communications, both to its Proceedings and to the Archæologia, were numerous and important. In 1892 he was elected President, thus becoming an *ex officio* trustee of the British Museum, and on his retirement from the post of Keeper of British and Mediæval Antiquities and Ethnography in 1896 he was elected a member of the Standing Committee of Trustees.

He was elected a Fellow of the Royal Society in 1874, and was an Honorary Member of the Royal Irish Academy and of numerous Foreign Societies. He also for some years before his death held the post of Antiquary to the Royal Academy. The University of Oxford conferred upon him the honorary degree of D.C.L., and that of Cambridge of Litt. D., and in 1894 he was promoted by Her Majesty to the rank of K.C.B.

Among British archæologists there was no one who had a wider or more accurate knowledge in nearly every department than Sir Wollaston Franks, and he was ever ready to communicate all that he knew with characteristic liberality. It is, however, sad to think what an amount of incommunicable knowledge has perished with him. His kindly nature led to the formation of numerous and enduring friendships, and his death leaves in the hearts of many a void that can never be refilled.

J. E.

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